TECHNICAL PROBLEMS AND THE GENEVA TEST BAN NEGOTIATIONS

HEARING

BEFORE A

SUBCOMMITTEE OF THE COMMITTEE ON FOREIGN RELATIONS UNITED STATES SENATE

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THURSDAY, FEBRUARY 4, 1960

U.S. SENATE,
SUBCOMMITTEE ON DISARMAMENT OF THE
COMMITTEE ON FOREIGN RELATIONS,
Washington, D.C.

The subcommittee met, pursuant to call, at 3 p.m., in room F–39, U.S. Capitol Building, Senator Hubert H. Humphrey (chairman of the subcommittee) presiding.

Present: Senators Humphrey, Gore, Church, and Hickenlooper.

OPENING REMARKS OF THE CHAIRMAN

Senator HUMPHREY. This is the Subcommittee on Disarmament of the Committee on Foreign Relations. I shall make a brief opening statement and then we will hear from our witnesses.

The Disarmament Subcommittee is meeting in public session this afternoon to discuss the status of a control system to monitor an agreement for the cessation of nuclear weapons tests. Of primary concern to us today are technical issues involved in the problem of adequate control. Our chief witnesses, therefore, are two prominent scientists who have had a major responsibility in developing an effective system for the control of a ban on nuclear weapons tests. We have anticipated that questions of policy will also be raised so I asked the Secretary of State if his Special Assistant for Disarmament and Atomic Energy, Mr. Philip Farley, might be present and Mr. Herter has kindly consented.

The nuclear test ban negotiations have been in progress for about 15 months. One month the prospects for agreement with the Soviet Union appear good. The next month they look bad. Recently the prospects took somewhat of a turn for the worse. A technical working group which met from November 25 to December 19, 1959, failed to reach agreement on the significance of new data regarding the efficacy of a control system to detect underground nuclear tests.

PURPOSE OF THE HEARING

Our purpose at this hearing is to explore the technical areas of disagreement with the Soviet Union, to review the remaining technical and political issues that stand in the way of a treaty, and to learn the present status of research to improve our ability to detect underground explosions.

I would like to stress the importance of conducting a review at this time of the technical and political problems which must be resolved before a test ban treaty can be signed. The Senate must give, by a two-thirds vote, its consent to the ratification of any treaty. Such a large majority means that no treaty can be ratified without strong support by both political parties.

A test ban treaty, if one is signed, will be the first treaty dealing with the problem of disarmament since the end of World War II. The Senate today cannot sit back and wait for a treaty of this nature to be negotiated and signed by the President before attempting to understand its provisions. Too often in the past our choice in the Senate has been either to reject a treaty the executive branch has labored long and hard to negotiate or to act as a rubber stamp and accept it without question.

ROLE OF THE SENATE

I might add that the constitutional provision of advise and consent includes the two words "advise" and "consent," and we prefer to do some advising before we are ready to do the consenting.

The Senate must be consulted constantly during the course of the negotiations. It must even, I think, indicate occasionally its views with respect to the course being followed by the executive branch. Last year, for example, the Senate unanimously went on record to endorse the objective of an agreement to end tests of nuclear weapons. I refer, of course, to the passage of Senate Resolution 96. That resolution specified, however, that adequate control measures must be part of any treaty.

What we face now is an evaluation of what exactly constitutes "adequate control." This is one of the major questions with which the Senate will be faced if and when any treaty is submitted to it. It is my view that the Senate must have a greater understanding of this question now. We cannot wait and pretend the sole responsibility for the adequacy of a treaty rests with the executive branch. Hence, I believe that hearings such as this one today are a vital part of the treaty ratification process. It is my hope that many of my colleagues in the Senate, as well as interested citizens, will take the time to read the transcript when it becomes available.

INTRODUCTION OF WITNESSES

I shall introduce our three witnesses all at once because the two scientists wish to speak as a team.

Mr. Farley, am I correct in saying that you have no prepared statement, but you will be available to address yourself to any of the policy or political questions that may arise?

Mr. FARLEY. I would be very happy to answer any questions you would care to raise.

Senator HUMPHREY. You have no prepared statement?

Mr. FARLEY. No prepared statement.

Senator HUMPHREY. The scientists, as I understand it, will restrict their comments, by their own wish, to technical matters.

Our first witness will be Dr. James B. Fisk, a member of the President's Science Advisory Committee and president of Bell Telephone Laboratories. During the 1958 Geneva Conference of Experts, Dr.

Fisk served as chairman of the U.S. delegation and, I might add, did an excellent job. We surely feel deeply indebted to him.

He also served as head of the U.S. delegation to Technical Working Group Two of the Geneva Test Ban Conference which was held from November 25 to December 19, 1959, to review problems concerned with the detection of underground nuclear explosions. Dr. Fisk has also served on the Atomic Energy Commission's General Advisory Committee, and he is well known to our colleagues here in the committee.

Our other prominent scientist who will testify is Dr. Wolfgang K. H. Panofsky who, in addition to being director of the High Energy Physics Laboratory at Stanford University, is also a member of the President's Science Advisory Committee. He was deputy head of the U.S. delegation headed by Dr. Fisk to Technical Working Group Two, and he was head of the U.S. delegation to the Technical Working Group on the Detection and Identification of High Altitude Nuclear Explosions. Dr. Panofsky is also a consultant to the Atomic Energy Commission.

As I said, our third witness is Mr. Philip J. Farley, special assistant to the Secretary of State for disarmament and atomic energy.

Dr. Fisk, I gather you would like to lead off?

STATEMENT OF DR. JAMES B. FISK, HEAD OF U.S. DELEGATION TO TECHNICAL WORKING GROUP TWO, GENEVA TEST BAN CONFERENCE

Dr. FISK. Thank you, Senator Humphrey.

I hope we will be able to help, at least in clarifying some of the technical problems that we are faced with.

In addition to Dr. Panofsky and myself, we have with us two other members of our delegation, Prof. John Tukey, of Princeton University, a very skilled mathematician and statistician; and Dr. Romney of our Department of Defense, one of our most distinguished seismologists.

Senator HUMPHREY. We are very happy to have them with us.

Dr. FISK. Inasmuch as our entire delegation, not only the four of us, but our entire delegation, was party to all of the discussions and all of the conclusions which we came to, I would like, if I may, to share the questions and to call on one or another person who is best qualified.

MAJOR AREAS OF DISAGREEMENT BETWEEN UNITED STATES AND U.S.S.R.

Now, we had from you, sir, a number of questions, six to be specific, most of them with substantial technical content and, if it pleases you, I would like to take the first one myself, which reads this way:

What are the major areas of disagreement between the United States and the Soviet Union on the detection and identification of underground nuclear explosions and earthquakes?

Senator HUMPHREY. Are you referring now, Doctor, to the communication that was addressed to you in reference to this hearing?

Dr. FISK. Yes. I am taking the first question.

Senator HUMPHREY. Thank you.

Dr. FISK. At our recent meetings in Geneva, we were given a charge by the main political conference, and were able to reduce that to a working agenda with the Soviets.

Our agenda contained several items, the first important one of which was this: Consideration of all data and studies relevant to the detection and identification of seismic events and of possible improvements of the techniques and instrumentation. That was one important element of our discussions at Geneva.

The other key item on the agenda was the consideration of the question of use of objective instrument readings, in connection with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine the basis for initiating on-site inspection.

Now, I would like now to come to the areas of disagreement. I think everyone knows that in the course of our four weeks of meetings, while we discussed many of the subjects, we did not agree on a very substantial amount of the material discussed.

AREAS OF AGREEMENT

The major areas of disagreement between the United States and the U.S.S.R. are best understood in terms of the Russian policy as revealed by the position which they upheld firmly throughout the entire negotiations of Technical Working Group Two. The Russians agreed with us on all statements which would imply that the problems of detection and identification are now as easy or might be made easier than was indicated by the 1958 Conference of Experts report.

They agreed on all proposed improvements to the Geneva system. Specifically they approved of the incorporation of additional seismic instruments at the control posts, and also, after considerable discussion, agreed to changes in classifications of the instruments already proposed by the 1958 Conference.

SOVIET VIEW OF 1958 FINDINGS

The Russians disagreed with us on, or considered premature, any matters which would appear to deteriorate the performance of the system.

In this respect the Russians emphasized continually that the 1958 report was a document "agreed to by scientists and approved by governments." And, therefore, in many instances they denied even the right of the technical working group to disagree with the experts' conclusions.

It is easily seen that agreement on purely scientific matters, in the face of this legalistic attitude, was extremely difficult, if not impossible.

Now, the specific areas of disagreement are these; I have listed six. The first, the estimate of the number of earthquakes equivalent to explosions of a given yield. This was really the content of another question, Senator Humphrey, and I hope we will return to that shortly, and Dr. Romney will elaborate on that, with your permission.

The second area of disagreement has to do with the evaluation of the measurements of the direction of first motion—the first earth motion in a seismic event, an earthquake or explosion, made during the Hardtack series a year ago last October.

VALIDITY OF "BIG HOLE" CALCULATIONS

The third area is the validity of the decoupling calculations, the big hole, if you like, that has had so much prominence, by which the seismic signal may be very substantially reduced from what it would otherwise be. They disagreed, particularly as to the adequacy of the calculation in predicting what would occur if decoupling were tried. They disagreed also on the feasibility of doing this.

The fourth area, evidence for the occurrence of unexpected surface phenomena, surface modes, if you like, as observed in the Hardtack tests. This was not extensively discussed, and it does not appear as a major item of disagreement. Nevertheless, it was an area of disagreement.

ADEQUACY OF SEISMOGRAPHS USED IN HARDTACK TESTS

The fifth area of disagreement, the adequacy of the actual seismograph, used by our delegation to study the detection used by our experts, to study the detection of the direction of first motion during the Hardtack series.

Now, the Soviet delegation criticized the actual seismographs which were used, claiming that since these did not conform in detail to the specifications laid down by the experts in 1958, the conclusions reached by the United States were not valid and the performance of the experts' system would, by definition, have been superior. Now, when it was shown in our meetings that seismographs, whose behavior the Russians claimed to be appropriate, would have been less effective than the ones we actually used, the Russians did not pursue the matter further during the technical working group meetings. Nevertheless, they reiterated this point in their annex to the final report.

Now, the sixth and final area of disagreement—and it is a very important one—was this: The principles on which a system of so-called objective criteria should be established to define eligibility of suspicious seismic events for on-site inspection. The Russians took the position that any criteria formulated by the technical working group must leave only a small number of events eligible for inspection.

Senator HUMPHREY. Excuse me. Would you repeat that, Dr. Fisk?

Dr. FISK. Yes.

SOVIET POSITION ON ON-SITE INSPECTIONS

The Russians took the position that any criteria formulated by this technical working group must leave only a very small number of events eligible for inspection. They set this as their charge, irrespective of the technical situation.

Senator GORE. What do you mean "their charge"?

Dr. FISK. Well, we were left with the strong impression that unless they could achieve this particular end, namely excluding almost everything from inspection, they would not accomplish the end to which they had been directed.

Senator HUMPHREY. You mean on-site inspection?

Dr. FISK. Eligibility for on-site inspection.

Senator GORE. You mean this was a political direction they had?

Dr. FISK. This is our surmise; yes, sir.

Senator GORE. This reminds me that Congressman Holifield and I undertook diligently at a dinner meeting to draw from Mr. Tsarapkin some estimate of the number of inspections which he thought would be adequate or acceptable.

He would never suggest a number, and I remember Congressman Holifield said, "Well, what about 15?" Mr. Tsarapkin said, "Too many."

Dr. FISK. We never discussed any actual numbers, Senator Gore.

Senator GORE. Could I ask one further question, Mr. Chairman?

Senator HUMPHREY. Yes; please do.

NO REFERENCE TO SOVIET UNDERGROUND NUCLEAR TESTS

Senator GORE. In all of the proceedings which I witnessed there, and the conferences held, I was never able to hear one single reference from the Russian delegates or staff which indicated that they had or had not conducted underground tests. Did you hear such references, statements, or allusions during the course of the technical conference?

Dr. FISK. There were no allusions whatsoever that I am aware of to the conduct of any underground nuclear tests.

They did make reference on several occasions to underground high-explosive tests, in connection with, for example, mining operations, and things of that kind.

Senator GORE. So that, insofar as you know, in the proceedings and the conference of experts, in the first instance, and in the political conferences that have ensued and also the experts' conference which you attended, no reference has been made to Soviet underground nuclear tests.

Dr. FISK. None whatsoever; no, sir.

Senator GORE. Didn't you find that interesting?

Dr. FISK. Yes.

Senator GORE. Well, as a nonscientist, I did, too.

Dr. FISK. Shall I continue?

Senator HUMPHREY. Proceed.

You are still listing, Doctor, the areas of disagreement between the United States delegation and the Soviet delegation, is that correct?

Dr. FISK. Yes, sir.

I have listed six, and I was elaborating just a trifle on the sixth which has to do with the objective criteria for eligibility to on-site inspection.

Senator HUMPHREY. Might I add that the Russians did not exclude, however, in the discussions the subject of on-site inspection.

Dr. FISK. No; it was not discussed. It was only brought up by our delegation as a specific item under new studies and data.

SOVIETS PROPOSE SMALL NUMBER OF INSPECTIONS

Senator HUMPHREY. My point was, did the Russians indicate that there would be no on-site inspection or did they indicate that there could be some on-site inspections?

Dr. FISK. This subject was not discussed explicitly in those terms, sir. But the implication was, of course, that there would be on-site

inspection under properly qualified conditions; and it was to try to establish those conditions that is the basis of this current discussion.

Now, the Soviet delegation proposed actually a set of criteria which would have made only a small number of events eligible for inspection.

But in so doing their criteria would have classified all nuclear explosions of the Hardtack series as earthquakes; that is, they would have been ineligible for inspection.

Now, interestingly enough, the Soviet delegation did not include their proposed criteria in their final report. We felt that this was rather significant.

Senator HUMPHREY. On what basis?

Dr. FISK. Why did we feel it was significant?

Senator HUMPHREY. Yes.

Dr. FISK. Because I think that anyone looking at their criteria would have recognized that they were completely unsuitable.

Senator HUMPHREY. So you feel that your presentation did make some impression, at least, on the scientific mind?

CALL CRITERIA "ON THE BRINK OF ABSURDITY"

Dr. FISK. Well, actually, if we quote the verbatim record which is here in several pound-sized volumes, the Russians challenged our criteria as being on the brink of absurdity.

I feel that theirs were really on the brink of absurdity for just the reason I have cited; namely, that the Hardtack series of explosions would have been classified as earthquakes under their criteria.

U.S. POSITION ON FORMULATION OF CRITERIA

Now, to state our position, it was simply that criteria should be formulated as such that within our present technical knowledge the events eligible for inspection should include a large fraction or all, if possible, of any nuclear explosions that might be detected.

As a result of this assumption, our criteria would retain in the eligible-for-inspection classification a very large fraction of the detected and located events.

SCIENTIFIC DELEGATIONS DIFFER ON APPROACH

Now, the last disagreement—this one about criteria—clearly illustrates the basic difference in approach of the two scientific delegations.

The Russians demanded that scientific facts be found which would serve a political necessity.

The U.S. delegation worked very hard to convert the actually known scientific into an acceptable set of criteria.

Senator GORE. Mr. Chairman, in that direct connection could I read one paragraph or sentence made by Mr. Tsarapkin?

Senator HUMPHREY. Proceed.

Senator GORE (reading):

> **The Soviet approach was different. We consider that the end purpose of criteria is to select a minimum of events suspected of being nuclear explosions and, of these events, to choose perhaps a few dozen for on-site inspection at the discretion of one of the parties.**

And you say your scientific objective was contrary to this?

Dr. FISK. Well, our objective was to go as far in that direction as the scientific facts would allow us at the present time; and the scientific facts, unfortunately, do not allow us to go very far today.

USE OF DIAGNOSTIC AIDS TO IMPROVE DETECTION

Now, it may be worth turning to a very closely related question, namely, the so-called diagnostic aids, those things which could be used, and may ultimately be capable of being codified into specific criteria, and would improve the system. I think this is closely enough related to this last point so that if it meets with your approval, Mr. Chairman, I would like to ask Professor Panofsky if he would talk a bit about that subject.

Senator HUMPHREY. May I add that these aids, to which you refer, relate to some of the suggestions in the Berkner report.

Dr. FISK. They include most of them; yes, sir.

Senator HUMPHREY. For the improvement of the detection system?

Dr. FISK. Yes.

Senator HUMPHREY. Dr. Panofsky, go right ahead.

Senator GORE. Mr. Chairman, before he proceeds, let me say, although he may not have been aware of it, I was in the Chamber during one period when the doctor was performing, and he did an admirable job.

Dr. PANOFSKY. Thank you.

Dr. FISK. He did on all occasions.

Senator HUMPHREY. Doctor, proceed.

STATEMENT OF DR. WOLFGANG K. H. PANOFSKY, DEPUTY HEAD, U.S. DELEGATION TO TECHNICAL WORKING GROUP TWO, GENEVA TEST BAN CONFERENCE

Dr. PANOFSKY. Senator Humphrey, you submitted a question to us, No. 3, which reads as follows:

The U.S. delegation's report concerning Technical Working Group Two lists several areas of research as well as diagnostic aids which might prove promising to improve our ability to detect and identify earthquakes. What are your assessments regarding these possibilities?

Now, before commenting on this in detail, let me just repeat, and possibly elaborate on, the remarks made by Dr. Fisk. The general process of control which is envisaged here is as follows:

Firstly, there are a large number of detected events. These are, by definition, suspicious.

PROBLEM OF REDUCING NUMBER OF ELIGIBLE EVENTS

We were trying to codify rules for reducing the number of events eligible for inspection below the number of suspicious events. It was in this area where the conflicts arose; namely, the Russians felt that science must be subservient to the needs of the moment to get the number of eligible events down; and we felt this number can be gotten down only if in the process of writing such a rule we would not run the risk of classifying the majority of potential explosions as earthquakes.

Now, the sad situation in this connection is the fact that our scientific knowledge in this general area is in a rapid state of flux. You cannot produce rigorous codes or rules on the basis of things which you do not know, but where you can just surmise that certain results might be forthcoming in the distant future.

Therefore, in terms of precisely known facts we were able only to formulate criteria which would still leave a very large area of events eligible for onsite inspection.

On the other hand, we were aware, and the Russians were aware, of a very large body of seismological information which, to a skilled seismologist, could be used to make an intelligent choice among these various eligible events.

However, this choice can only be made by a skilled seismologist.

We had absolutely no information which enabled us to write a rule book as to how precisely such information should be used, simply because of the fact that most of the work on this problem is in its infancy.

VIEWS DIFFER ON HOW MUCH DIAGNOSTIC AIDS WOULD IMPROVE CONTROL SYSTEM

I consider the question of the diagnostic aids to be one of the most important matters discussed by the Technical Working Group.

However, these aids make use of seismic phenomena with relationship to earthquakes or explosions which are not well enough known to enable them to be codified.

Now, evaluation of how much their use would improve the capability of the system depends to a great extent at this point on judgment, and this judgment should be expressed by competent seismologists, and I am not a seismologist.

However, I have attempted to canvass the opinion of many seismologists, including Dr. Romney here, as to their judgment, and I find that the judgment differs a great deal.

Some seismologists believe that the number of eligible events could be reduced greatly by the information; some believe it cannot.

Under all conditions the answer would depend a great deal on the degree of talent which would be available to the control organization in making this choice.

If no very high-class seismological talent is available, such information would not be useful.

QUESTION OF RECRUITING SEISMOLOGISTS

Senator HUMPHREY. May I interrupt? What do you mean by "would be available"? Are you indicating we do not have a substantially large body of seismologists of this competence, or are you indicating that they might not be recruited for such an international inspection project?

Dr. PANOFSKY. I feel the latter. At least, one cannot prejudge the matter how easy it would be to get the very large number of seismologists required. Considering the very large body of data which would have to be handled, I do not believe it is obvious that a large and skilled enough body can be recruited.

CONDITION UNDER WHICH AIDS WOULD BE USEFUL

However, in my opinion, the availability of the auxiliary aids constitutes a clear mandate to explore to the fullest the possibility of a unilateral choice of the events to be inspected.

It is my opinion that if the kind of system existed where one nation is permitted to make a selection of inspections on the other nation's territory, in that case these auxiliary aids might be quite powerful.

However, I am, unfortunately, unable to put in quantitative terms a statement as to how much more powerful they would be or how soon they would become more powerful.

This depends on trying to forecast the efficacy of the future research program, and this is a very difficult thing to do.

Senator HUMPHREY. May I interrupt again?

Dr. PANOFSKY. Yes.

TRAINING SKILLED SEISMOLOGISTS

Senator HUMPHREY. Are you saying that there are not enough trained, competent, highly skilled seismologists to properly man an adequate inspection system as of now?

Dr. PANOFSKY. It depends on what they have to do.

If each suspicious event has to be a subject of detailed critical examination, then I think the answer is there are probably not enough.

Senator HUMPHREY. Could you train enough?

Dr. PANOFSKY. In time, I presume so, yes. I do not know.

This question probably should be answered by my seismologist colleague. I think it depends on how hard you make the job.

I asked one of my colleagues once the following question:

If on one event you asked a student to write a doctor's dissertation, how sure could you be whether it was an explosion or an earthquake. He gave a fairly optimistic statement. But this means a very large body of investigation.

On the other hand, if it had to be routine procedure the answer is very different.

Senator HUMPHREY. You are speaking of the seismologist without the auxiliary aids?

Dr. PANOFSKY. No, with using the auxiliary aids.

Senator HUMPHREY. Even with the use of auxiliary aids it would be very difficult?

Dr. PANOFSKY. Well, using the auxiliary aids it would be more powerful, but also more difficult.

Without the auxiliary aids we are attempting to write a code book which does not require the greatest of skill, but can be reduced to a more routine procedure, that is what the criteria process intended to do.

With the auxiliary aids you need a higher degree of skill simply because of the fact that a routine code book cannot be written. The situation will change, will improve, and one of the possibilities does, of course, exist, that some of the items which are now auxiliary aids will graduate, so to speak, into rigid criteria.

But again, I find it dangerous to predict a time scale in which this development can take place.

I do hope, however, that both in the interest of pure science and also in the interest of the solution of this particular problem that the efforts to increase our knowledge in this area can be increased.

Senator CHURCH. Mr. Chairman, may I ask a question or two at this point?

Senator HUMPHREY. Yes.

POSSIBILITY OF RELIABLY DETECTING UNDERGROUND TESTS

Senator CHURCH. Apropos of your last remark, Dr. Panofsky, and of your earlier remark that "Our scientific knowledge is in a rapid state of flux," and the still earlier remark by Dr. Fisk that "Scientific facts don't allow us to go very far today," let me ask this question:

Based upon the data that we have accumulated from the subterranean tests that we have thus far carried out, do you think that it is possible to devise a detection system that would be reliable for the purpose of detecting underground tests? My assumption is that our criteria for such a system would be accepted.

Dr. PANOFSKY. I think the answer to your question depends on two facts:

One is a matter of political judgment as to what you mean by reliable, namely, what chance of detection is an effective deterrent against carrying out tests. As you know, there is a wide spectrum of opinion on this.

However, I would also answer your question as follows: It is certainly possible in principle to devise a reliable system. The problem is that we are trying to evaluate the particular system which was designed by the Conference of Experts in 1958.

Whether it is politically possible to devise such a system is a different question since it would involve expanding the system of the experts. The present less optimistic assessment of the performance of the experts system is a quantitative matter, and one could recapture, even with present knowledge, some of the lost performance if we simply introduced more stations.

However, if one restricts one's self to the network of stations which was recommended and discussed by the experts, then the question whether it is reliable or not would depend on whether the very marginal performance for low-yield explosions and the problem engendered by the big hole do or do not make the system unreliable in your political judgment.

Senator CHURCH. I believe I am following you, Doctor.

It is my understanding that the Russians maintain that they have never conducted any subterranean tests, and we have never charged that they have conducted such tests. That being so, the Russians must be depending, for purposes of these negotiations, upon the data that we furnish based upon our experience with subterranean tests.

Senator GORE. Would you yield there just for a moment?

Senator CHURCH. Yes.

NO EVIDENCE OF SOVIET UNDERGROUND NUCLEAR TESTS

Senator GORE. I had not understood that the Russians had said they had not conducted underground tests. I understood that they

had said neither yea nor nay as to whether they had or had not conducted such tests. I ask which is correct?

Dr. PANOFSKY. They have never introduced any evidence of any kind that they had.

Maybe some of my other colleagues can amplify this. I do not recall that they have ever denied having carried out such tests explicitly; I do not know.

Senator CHURCH. Can anyone answer that question before we move on?

Senator GORE. Yes.

Senator CHURCH. I think we should know. Have we ever inquired of them in the course of these negotiations in the past 15 months whether they have conducted subterranean tests?

Dr. FISK. There has always been a strong implication they have not. Whether they have ever made a positive statement on the record that they have never conducted underground tests or not, I do not believe there is that——

Senator CHURCH. Have we ever asked them the question, Mr. Farley?

STATEMENT OF PHILIP J. FARLEY, SPECIAL ASSISTANT TO THE SECRETARY OF STATE FOR DISARMAMENT AND ATOMIC ENERGY

Mr. FARLEY. I am afraid I cannot recall that we ever asked them the specific question or that they volunteered a negative, but I think the record does show very clearly this implication that Dr. Fisk refers to, that they have no experience with the subterranean nuclear explosions.

Senator CHURCH. Don't you think that would be a pertinent question to ask?

NO SOVIET DATA OFFERED ON UNDERGROUND NUCLEAR TESTS

Dr. FISK. The question has been asked, in a sense. We have asked them many times, "Do you have any data directly relevant to this particular point?"

It seems to me that a flat no on that constitutes a pretty good answer to the question.

Senator CHURCH. In any case, I am right in my assumption that the data which have been used as the basis for the negotiations relating to subterranean tests have been our American data.

Dr. PANOFSKY. That is correct.

There is a small qualification. There is some theoretical input on the part of the Russians, and also some data-taking in Russia with Russian instruments from the American explosions.

CHANGE IN SOVIET ATTITUDE TOWARD U.S. DATA

Senator CHURCH. Has there been any hesitancy on the part of the Russians to accept the data which are essentially American data? Have they raised any questions with respect to their feeling that it may not be reliable or that they may question its reliability? Have they been in the main ready to accept our data and ready to negotiate on the basis of our data? That is the thing I am trying to get at.

Dr. PANOFSKY. In the beginning of the negotiations they made it very clear that they would accept our data freely. They would use such phrases as "We have the greatest respect for your scientists, and we have confidence in your data."

However, during the last 2 or 3 days when it became clear that there would be disagreement on these conclusions they changed somewhat on this, not in a very strong way, but they used—I can quote verbatim, if you will permit me to look for it for a moment—they used such phrases as "The American data have been viewed with a larger degree of caution"; they used phrases of that kind.

Yes, here in the Russian annex to the report it reads as follows:

> Notwithstanding the fact that the initial processing, the preparation of analysis and the analysis itself of the seismic data had been done by persons who were U.S. nationals, the Soviet scientists tried to approach the material with the utmost confidence in its reliability under the assumption that it was the result of careful scientific work.

But then they continue:

> The above-mentioned manipulation of the prior data, the erroneous findings and the documents presented by U.S. experts and the fact that instrumentation used for observing the tests was not in conformity with 1958 Geneva recommendations have compelled the Soviet scientists to adopt a more cautious attitude toward the quality and objectivity of the figures that were supplied by the Americans.

It was this sentence which, as far as we were concerned, constituted quite a departure from their past attitude. In the past, I believe it is true, they had repeatedly said that they had the utmost confidence in our work, and this sentence was probably one of the most disturbing outcomes of these discussions to us.

Senator CHURCH. I can appreciate that, Doctor.

NEED FOR RESEARCH WITH AND WITHOUT UNDERGROUND NUCLEAR EXPLOSIONS

Let me ask you another question.

It is my understanding that we have conducted only four or possibly five subterranean tests. When you say that our scientific knowledge is in a rapid state of flux, do you mean by that that if additional tests were taken under differing conditions that the data that we might gather from these additional tests, compared with the data we already had, might firm up our knowledge of what, in fact, may be required for a reliable detection system?

Dr. PANOFSKY. The research which is needed is in several categories.

A very large body of it does not require the carrying out of additional underground nuclear explosions. It requires mathematical analysis, explosions carried out with conventional chemical explosives, earthquake studies, and many other such developments.

Certain tests, however, would require additional nuclear explosions.

PROPOSAL MADE BY SENATOR CHURCH

Senator CHURCH. About 3 weeks ago I made a proposal to the State Department that if we could reach agreement with the Soviet Union on a ban relating to all types of tests that contribute fallout to the atmosphere—spatial tests, atmospheric tests, and underwater

tests—and concurrently come to an agreement with them to conduct some joint experiments, perhaps under the auspices of the proposed International Control Commission, or perhaps under the auspices of the United Nations, this might give us an opportunity to work out the questions that are now threatening to stalemate the Geneva negotiations on a basis that would prove mutually satisfactory to all parties concerned.

Can you give me the benefit of your judgment with respect to the usefulness of that suggestion?

Dr. PANOFSKY. I can give you an answer regarding the technical usefulness.

I think from the point of view of its technical utility, the answer is very decidedly a positive one. Carrying out future research, both not involving and to some extent involving nuclear explosions, jointly or separately, that does not change the technical aspect, is clearly a step in the right direction.

The problem is to put this into an acceptable political framework; and, in particular, I would consider it dangerous to make a specific assumption as to a fixed time period in which such a program would resolve these difficulties.

I think nobody could responsibly predict how long it would take until the necessary results were obtained.

But technically I would say there is no question whatever that this is an extremely constructive approach.

Senator CHURCH. Thank you.

I did not mean to take so long, Mr. Chairman.

Senator HUMPHREY. That is all right. It is a good point.

TECHNICAL ASPECTS OF THE BIG HOLE THEORY

One of the questions that constantly comes out in testimony relates to the so-called big hole theory or the muffling of underground tests. This is written about and talked about, but I am not sure that all of us understand it. Between Dr. Fisk and yourself, and Dr. Romney and others, maybe we can get a little more enlightenment.

I call to your attention the report of the Berkner panel which indicated certain ways that underground explosions might be concealed. I speak now of nuclear explosions. They spoke about the concealment of explosions by the muffling of the seismic signals, and then the Rand Corp. made an estimate that a 300 kiloton nuclear explosion can be made to look like a 1 kiloton nuclear explosion if certain conditions are met. This all relates to this decoupling theory and the so-called big hole operation.

I have a few technical questions here. First, what conditions must be satisfied in order to muffle a nuclear explosion, that is, from a technical point of view? Can you give us some idea of how difficult or easy it would be to meet these conditions for muffling? Maybe I should give you a little more on this, because I think you can address yourself to this whole subject matter with a little more description on my part of the interest that I have in it.

The engineering studies by the Rand Corp., I believe, indicated that a nuclear shot as high as 70 kilotons could be concealed. So I ask how big a hole must be dug per kiloton shot, and how big a hole

would be needed for a 70 kiloton shot? Then what about the costs involved, the time involved, and the use of salt to cushion the shot?

Now, just taking all of those factors into consideration, I would appreciate your giving some attention and some description to what we are talking about here.

Senator GORE. May I add one additional factor?

Senator HUMPHREY. Yes.

Senator GORE. Which is the geographic distribution——

Dr. FISK. Of salt?

Senator GORE (continuing). Of the detection centers?

Dr. FISK. Yes. There is an awful lot here.

Senator HUMPHREY. I wanted to give you the total dimension of my interest in it, and then we will come back to the details.

THE BIG HOLE THEORY

Dr. FISK. Shortly before you arrived, we made a little calculation about the volume of this room, just to illustrate this point.

If this room were, roughly, a half-mile underground in salt, our rough estimate would be that a nuclear explosion about one-seventieth of a kiloton could be exploded inside of it, and the signal would be reduced by one or two hundredfold.

Or, if you like, a room 70 times this volume would be required, according to the theory, to contain 1 kiloton.

Senator HUMPHREY. Let us just get that straight. In other words, a room 70 times as big as this room——

Dr. FISK. In volume.

Senator HUMPHREY. (continuing). In volume, one-half mile under the surface——

Dr. FISK. Roughly.

Senator HUMPHREY. (continuing). Roughly, in salt——

Dr. FISK. Or hard rock.

Senator HUMPHREY. (continuing). Or hard rock, would be required to muffle or conceal a 1-kiloton explosion?

Dr. FISK. This is what the theory says.

Senator HUMPHREY. That is the theory.

BRITISH APPRAISAL OF BIG HOLE THEORY

Dr. FISK. And the theory holds up very well under every critical look that our people or the conference in November and December in Geneva was able to give it.

Now, I think you might be interested, Senator, in the appraisal of this particular part of our conference in Geneva, the appraisal that was given by the British delegation under Sir William Penny, and he states his conclusions this way. He says:

The theory shows that the seismic signal generated by a nuclear explosion in an underground cavity can be much less than that from an explosion of equal magnitude under Rainier conditions.

This was the first test.

The theory assumes that the cavity is sufficiently large and deep for the material around the cavity right up to the boundary surface to behave elastically.

Elastically means if you displace it, it comes back into the same form.

Calculations indicate, for example, that a cavity in salt or hard rock 1 kilometer—

roughly half a mile—

below ground level, of volume 70,000 cubic meters per kiloton—

that is where this room thing comes from—

would suffice to meet the theoretical requirements.

Then he goes on to say:

The theory assumes a perfect isotropic medium and makes certain assumptions about the stress distribution around the cavity before the explosion occurs.

He adds:

Careful work would have to be done before it could be stated that conditions likely to be met in practice would not require the theory to be modified. However, it might well be possible in practice should the necessary scientific research and engineering effort be found to give confidence that a decoupling factor of two orders of magnitude—

and by this he means a hundred or more—

compared with the Rainier conditions could be achieved.

I think there are two things that ought to be added to this: One is there have been some experiments, both in Britain and in the United States, with high explosives which tend to confirm the theory.

EXISTING CAVITIES MEET REQUIREMENTS FOR SMALL EXPLOSION

The second is that cavities are known to exist in salt formations which would satisfy the volume and depth requirements for a few kilotons.

Senator HUMPHREY. Where are these cavities?

Dr. FISK. I think I had better ask Dr. Romney.

STATEMENT OF DR. CARL ROMNEY, SEISMOLOGIST WITH THE AIR FORCE

Dr. ROMNEY. I could not tell you, I am sorry.

Senator HUMPHREY. If you know that they exist, you must know where they are. That is a fair assumption, is it not? Somebody must know.

Dr. ROMNEY. Yes, I am sure if you will ask the right people you will get the answer. I do not happen to know where they are.

Dr. FISK. There is salt in Louisiana and in the Texas area. There are zones there. I do not know either, Senator Humphrey, precisely where they are, but the information which we have from engineers, which we did not explore in detail in the technical working group, tells us that cavities of this size do exist.

Furthermore, they tell us that it is not too difficult to actually mine by hydraulic means and that holes of this sort are used to store gas and oil today.

Senator HUMPHREY. I was just wondering whether they existed in this hemisphere or whether they existed in the Asian area or where,

because if the cavities of which you speak exist here, we would not need to worry about a Soviet explosion in them.

DESCRIPTION OF HOLE NEEDED TO CONTAIN 70-KILOTON EXPLOSION

What I am getting at is, according to your testimony, a cavity of 70,000 cubic meters volume is needed to muffle a 1-kiloton explosion under these rather unique conditions of the elasticity, a half mile under earth in a location apparently that would be rather ideal. If you had a 70-kiloton explosion, which is what the engineering study indicated could be muffled, that would need a cavity of 4,900,000 cubic meters.

Dr. FISK. That is a big hole.

Senator HUMPHREY. That is a very big hole, is it not? How big would that be as compared with the Pentagon?

Dr. PANOFSKY. Smaller than the Pentagon, but not much.

Dr. FISK. At least 5,000 times as big as this room.

Dr. PANOFSKY. It would be a hole 600 feet in diameter.

Senator HUMPHREY. It is equivalent to two city blocks all the way around.

Dr. PANOFSKY. That is right.

Senator HUMPHREY. That is a bit of a job.

Dr. PANOFSKY. That is right.

TIME REQUIRED TO DIG A 70-KILOTON HOLE

Senator HUMPHREY. Do you have any idea what it would cost if you had to dig one of those? How long would it take?

Dr. PANOFSKY. Let me make the following remarks:

Firstly, the length of time has been estimated to be between 2 to 4 years to make a hole for 70 kilotons.

Senator HUMPHREY. Two to four years?

Dr. PANOFSKY. Right.

Now, let me make one other remark. We keep focusing our attention on salt.

Now, there is nothing magic about salt. The reason one talks about salt is because that is the medium in which engineers believe it would be the easiest to make such a big hole.

It is not the properties of salt which make the muffling better, but it is just the fact that salt appears to be the most economical way of producing such a hole.

Senator HUMPHREY. It would take 2 to 4 years, in other words, in the salt area——

Dr. PANOFSKY. Yes.

Senator HUMPHREY. And if you happened to run into something a little more difficult than salt, it would take longer.

Dr. PANOFSKY. It would take longer and cost more.

Senator HUMPHREY. Where do the salt areas of the world predominate.

Dr. PANOFSKY. Everywhere.

We know the Russians have large solution-mining operations and they are, therefore, familiar with the technique.

COST OF CONSTRUCTING UNDERGROUND CAVITY

Actually the question of natural occurring holes is not so critical because the natural occurring holes we know about are small. They are only useful for concealing explosions of 1 kiloton or so, which are difficult to identify anyhow.

No really thorough engineering studies have been made which give reliable cost figures, but just as rough guidance, several hundred thousand dollars per kiloton for the hole is the kind of figure which the engineers discuss.

This means that for 50 kilotons you might end up with figures in the general order of $10 to $30 million.

These were figures which were produced by a rather brief study of the Atomic Energy Commission.

Senator HUMPHREY. Was that the study under the Livermore Laboratories or the studies down at Louisiana?

Dr. PANOFSKY. I am not familiar with it. I was not involved in this, but they were contracted for by the AEC. I do not know whether they went through Livermore or not.

Senator HUMPHREY. We have known about the big hole theory since last March. In the meantime, the executive branch has persisted in its policy decision to negotiate for a complete ban on tests.

LETTER FROM THE PRESIDENT REGARDING RISKS OF EVASION

I received a letter from the President on November 17, 1959, which is substantially after the Berkner report and after the presentation of the big hole theory, and the President said:

> The best and the most carefully elaborated disarmament agreements are likely to carry with them some risks, at least theoretically, of evasion. But one must ponder, in reaching decisions on the very complex and difficult subject of arms control, the enormous risks entailed if reasonable steps are not taken to curb the international competition in armaments and to move effectively in the direction of disarmament.

Now, my question is: Does the President's statement mean that the executive branch has pondered the risks raised by the big hole theory and has concluded that the risks are not so great as to invalidate the objective of a ban on underground nuclear tests?

This comes right back to the policy question of how much a risk you are willing to take in light of both the practical knowledge that is available and the theoretical knowledge.

U.S. POSITION ON COMPREHENSIVE BAN

Mr. Farley, I suppose this reaches over into the area of policy decision and since there is considerable information, at least theoretical information, about the possibility of evasion of adequate inspection, particularly this big hole theory, why have we persisted in our negotiations for a controlled cessation of nuclear weapons tests on a rather comprehensive basis?

Mr. FARLEY. Senator, we have done this because it has been our objective from the first to obtain a controlled agreement that was as broad as possible, one which, if possible, would produce the control suspension of all nuclear weapons tests.

PROBLEM RAISED BY NEW DATA AND BIG HOLE THEORY

The situation when the new data so-called were developed early last year, and when the theoretical studies relating to the big hole were produced did, of course, throw into doubt whether it was now possible to safeguard a test suspension agreement which covered underground tests.

This was the problem which we, of course, raised early last year in the negotiations which led us to press very strongly all during last year for resumed technical discussions to see if it was possible to establish a technical basis, an agreed technical basis for continuing to negotiate for a comprehensive suspension.

It was only after a great deal of prodding and insistence on our part that the second technical working group, which these gentlemen are reporting on, was reluctantly agreed to by the Soviet Union.

DISAGREEMENT BLOCKS COMPREHENSIVE BAN

It was, of course, our hope that the facts would be looked at there, that answers would be found that would provide a technical basis for concluding a comprehensive agreement.

Unfortunately, we did not get the agreement which would make this possible.

As you know, we are now in the position of attempting to confirm whether the areas of disagreement which have been indicated here do indeed reflect the final attitude of the Soviet Union regarding the possibility of reestablishing an agreed technical basis.

Without it, our position would not permit at present concluding an agreement covering the underground area.

The big hole with its unsolved problem, is one of the reasons for this.

Senator HUMPHREY. In other words, are you stating that our Government has now changed its position?

Mr. FARLEY. Our position, I think, has been consistently that we are ready to negotiate an agreement to suspend all forms of testing which can be controlled.

The data and the theories, which we are referring to here, and the fact that we have not been able to agree to solutions to them, make it presently impossible to get an agreement which would give us adequately safeguarded suspension of underground tests.

We must have an agreed technical basis and a political agreement incorporating such controls in order to suspend the underground tests.

I do not think that is a fundamental change in position. The objective is the same. The possibility of reaching it at present is in doubt.

I would like to just comment on the question Senator Church raised earlier, because I think it may serve to clarify our attitude.

He asked, if you will recall, about the possibilities of remedying some of the present technical deficiencies.

EFFORT TO SOLVE TECHNICAL PROBLEMS

Our attitude is that we must make every effort we can to solve the problems that have arisen.

The question of whether they can be solved is one question we cannot answer.

The thing we can do, of course, is bend full efforts to the solution and to that end, as you know, there are extensive programs now underway to improve seismic detection, to further develop the systems which would enable us to overcome these difficulties.

We have these underway ourselves. We would, of course, consider it advantageous if not only the British but the Soviet Union had such program and if they could be coordinated, since not only would that redouble the effort but it would mean that we would, to some extent, overcome these problems of lack of confidence.

The question of objectivity, if we both had that which was pooled, which was examined objectively in the customary manner of the scientific community, that problem would be overcome.

Our objective is the same. We are trying to answer the problems which have arisen. It is only so long as we do not have agreed answers to those that we cannot agree to a complete suspension.

STATUS OF RESEARCH ON SEISMIC DETECTION

Senator HUMPHREY. May I add that this matter of the auxiliary facilities and of improved research was a part of the Berkner report that received little attention. The Berkner report recommended $10,825,000 for the first-year research program to improve and expand seismic detection, exclusive of another request of $12 million for the cost of nuclear and chemical detonations.

I have written to Dr. Herbert York, the head of Research and Engineering for the Department of Defense, and to Adm. Arnold Karo, the Director of the Coast and Geodetic Survey, to learn the status of the research on seismic detection. A letter to me from the Department of Defense states that $8,535,000 has now been allotted for research and development in the seismic detection field. This is approximately $2,300,000 less than the Berkner panel indicated as sort of a minimum requirement.

Could you give us some figure or some picture of all that is being done to step up our research activities in the field of detection? Why the $2.3 million reduction here? Why the reduction of the amount considered to be a basic minimum by the Berkner panel?

Mr. FARLEY. I think I could give you a general answer only, Senator, and perhaps Dr. Romney can supplement it on detail.

There is the seismic improvement program to which you refer undertaken by the Department of Defense which is supplemented also by experimental activities in the Atomic Energy Commission.

I would submit that the allocation of this amount of money outside the budget cycle is a real earnest of our intention to proceed rapidly.

This is a substantial additional sum to come in, and I believe it does represent a sincere effort to find funds and get a substantial program underway.

As to the details of the program, as I say, I think I am not the best witness.

Senator HUMPHREY. Senator Gore?

U.S. POSITION HAS NOT CHANGED

Senator GORE. Mr. Farley, with respect to the question of whether the U.S. Government has or has not changed its position, is it not a fact that the negotiating position and the goals to be sought were formulated and agreed upon prior to the Hardtack series and that the results of the Hardtack series were not fully calibrated and analyzed until after the Geneva Conference on the Discontinuance of Nuclear Weapons Tests had begun?

The new evidence that came to the knowledge of the Government made it necessary, in the administration's view, to insist upon a consideration of the data and of the questions and doubts, posed by the Hardtack series.

Therefore, the position which was originally taken was not necessarily abandoned, but it required a period of study and a more careful evaluation of means of detection, particularly of underground tests.

Have I thoroughly stated the chronology and the facts involved?

Mr. FARLEY. I think that is an accurate statement.

Senator GORE. Well, as you know, I have undertaken every reasonable way to cooperate with the administration in this vexatious field.

At first, I was impatient with the set of circumstances which I have described and which you have generously said I have described reasonably accurately.

Mr. FARLEY. Reasonably and accurately.

Senator GORE. Thank you, sir. I say I was impatient with it because I was briefed about this matter before going to Geneva, and I was there on the opening day. But I later came to realize that although I was briefed on it, the careful calculation and analysis of the data were not possible before the Conference had begun and, therefore, I do not believe that our Government has abandoned its goal or has changed its position.

It has insisted that the new data resulting from the tests and the analysis of those tests be taken into consideration and that adequate means of detection and control be a part of the agreement. Am I still reasonable and accurate?

Mr. FARLEY. Yes, sir.

Senator GORE. All right. Thank you, Mr. Chairman.

Senator HUMPHREY. I might, Mr. Farley, refer a couple of these questions to your colleagues in the scientific field.

CORRESPONDENCE RELATING TO SEISMIC RESEARCH PROJECTS

First, I think it might be well to include in the record the letters and responses which we received from the Coast and Geodetic Survey, the Department of Defense, and other departments of Government relating to questions concerning the research projects which are being contemplated or currently underway as the result of or in accordance with the recommendations of the Berkner report.

I addressed letters to Dr. York and to Admiral Karo, and I will ask that those reports, including the letters in response and the suggested research projects, be made a part of the printed record. They are very interesting reading.

(The text of the documents referred to is given in the appendix, starting at p. 51.)

SUGGESTED IMPROVEMENT IN SEISMIC DETECTION

Senator HUMPHREY. Two matters were brought out in the Coast and Geodetic Survey suggestions, that would improve detection. One was more accurate determination of the depth of earthquakes, and the other was the placing of seismographs in deep holes as a means of reducing background noise.

Dr. PANOFSKY. Dr. Fisk or Dr. Romney, do you have anything to say on that?

Dr. FISK. I think Dr. Romney should address himself to that.

Dr. ROMNEY. Well, concerning the matter of determining the depth or focus more accurately, it is certainly contemplated that there will be work done in this area as part of the work resulting from the recommendations of the Berkner panel.

I am not aware of any intensive work being done right now except work at the university laboratories as part of their normal research work.

Concerning the——

Senator HUMPHREY. Don't we have any grant projects with the university laboratories? Do we have any research projects on these matters, such as the Department of Defense has on a host of other subjects with these laboratories?

Dr. ROMNEY. I would presume you could get the answer on that more appropriately from the answer of Dr. York. But, to my knowledge, I know of no Defense Department efforts purely in this line.

Senator HUMPHREY. What I am getting at is that there does not seem to be much doubt as to the possibility that an adversary or the Soviet Union would attempt to use the so-called big hole operation to disguise an underground nuclear test, despite the tremendous engineering problems involved, the 2 to 4 years required under favorable conditions to dig a big enough hole at least for a 70-kiloton explosion, and the costs involved.

Since it is assumed that this would be done, why don't we assume, with the same zeal, that we would improve our research?

Dr. ROMNEY. I am sure this is the intention of the program which is now starting.

Senator HUMPHREY. Why don't we hear more about that? All I hear about are the problems involved. What about the solutions?

CONCLUSIONS OF BERKNER PANEL

Now, the reason I mention this is because the Berkner Panel which brought up all these problems and, I think, very rightly so, and indeed gave all of us reason to pause and to think through what was underway, also concluded, and I quote from it:

> That the program of research that it recommended would in 3 years probably result in further improvements which could achieve the same capability in the Geneva net of 180 stations as was originally estimated by the Geneva Conference of Experts.

In other words, the Geneva Conference of Experts felt at the time that it recessed it had arrived at a control or detection program that

was safe enough at least to enter into an agreement to ban tests. Later on it was found that the data were too scanty.

The Hardtack series revealed inadequacies and limitations in the original control system, and then the Berkner report came back and said that if you do certain things you can restore the capability that was assumed prior to the Hardtack series.

URGES MORE EMPHASIS ON IMPROVEMENT OF CONTROL SYSTEM

There is much negative weight given to the findings of the Hardtack series. We hear all about the big hole; we hear that the Russians can cheat; we hear they most likely will, and that they will not agree. I am not necessarily disputing any of this. All I am saying is why don't some of you emphasize what was in the Berkner report, namely, that with a little effort, with some more money and with some more information the Geneva control system can be improved. When we ask the questions about what we are doing to improve research, you should have the same alacrity that you have when relating the problems that we face. Why is it that we do not get this positive side of the picture, or is this side just soothing sirup to make us all feel happier? If it is, we ought to know that, too. Mr. Farley, do you want to get on this one first, because I want to stay with you on it for awhile.

Mr. FARLEY. Fine.

Let me make one comment and, perhaps, there will be others who will want to supplement it.

In part, Senator, I think you are asking a question of why some things get attention publicly and some things do not, and that is just an impossible one to answer.

I would point out that in the agenda of Technical Working Group Two both elements were emphasized on one part.

On the question of improvements, it was possible to get useful discussion and some areas of agreement.

My speculation would be that agreement is simply less exciting than disagreement, and so the disagreement gets more attention?

Senator HUMPHREY. Who gives it more attention?

Mr. FARLEY. Well, you see there are the two halves. One is the public question of why there is more discussion of one than the other. On that one about the best answer I can give you is that one makes more news than the other.

SUBSTANTIAL PROGRAM UNDERWAY

As to attention in the Government, the point which we have tried to emphasize is that we are getting work underway, and I would not claim that it has gone as fast as it should, but it is now a substantial program and it is underway.

It has been necessary to call attention to the uncertainties and the inadequacies that have been raised because they did affect the short-term negotiating objectives, and this point had to be clear if we were to focus on the problems that were before us in the negotiations.

Senator HUMPHREY. May I just interrupt there?

Mr. FARLEY. By all means.

Senator HUMPHREY. I do not deny this is a very important observation that you are making. But if you were just on the verge of what you thought was the discovery for the treatment of some disease and you ran into uncharted or unpredicted difficulties you would not just talk about the problems of the disease but you would accelerate the response to overcome the difficulties.

QUESTION OF ENTHUSIASM FOR SOLVING PROBLEMS

There is a great deal of knowledge about the problems involved here, and I think this is important. You must have this out in the open. It would be a sad thing if it were not. But where is the enthusiansm for overcoming the difficulties? Can you list for me 25 research projects which we are undertaking at the moment? Can you list five or two or one or three that we are really excited about as to the possibility of overcoming some of these difficulties?

If we cannot, then why are we talking about continuing the negotiations? It seems to me that if we are going to be experts in the big hole theory, which is the big bad wolf now, and if we are going to be experts on the fact that the Russians can cheat, which is the second big bad wolf, there ought to be some good fairy coming along very soon. There ought to be some good news around here, or somebody ought to be looking for it.

Is anybody really looking, or do we have somebody who is just one step away from eternity looking for it? Do we really have somebody who is alive, alert, and after it, pounding the Bureau of the Budget for money and pressing the Department of Defense to get busy, or are we just taking this in an easy stride?

Mr. FARLEY. As for the allocation of funds, I have already spoken, and I believe you have referred to Dr. York's reply.

On the question of whether we pound, I can certainly testify that I, together with the President's Office, have convened a meeting as recently as 10 days ago to be satisfied that a program was being developed.

I repeat that I am not a good witness to speak to the specific programs and their timing within the 3-year-time scale which, as you recall, was identified by the Berkner Panel.

I do not know whether my colleagues here can answer as to specific projects.

Senator HUMPHREY. Dr. Panofsky, do you have any information?

SCIENTIFIC HONESTY OF U.S. NEGOTIATORS

Dr. PANOFSKY. I think I would like to state that I am one of the ones who has been disturbed in these negotiations about just these points you raise.

During the negotiations we felt that as responsible representatives of the U.S. Government we had to do a scrupulously honest job in terms of the scientific facts as they were available, and that we were in no position to question the problem which was and is caused by the fact that we wish more facts were available. That we cannot do and still be responsible negotiators.

I might say that from the point of view of scientific honesty, I think we have completely and entirely a clean record.

U.S. RESPONSE TO RESEARCH NEEDS

On the other hand, I agree with you that there does not exist at present an efficient enough mechanism within the Government for carrying out the necessary scientific work to back up the constructive aspects of these problems.

The difficulty is that as soon as a difficulty, such as the big hole develops, this is immediately a problem and the burden of proof, so to speak, that the problem is not serious, rests then on further research. Our response regarding such further research is, to me at least, discouragingly slow. In many instances I believe that the statements which were made by the Berkner Panel, that research will definitely restore the capability of the system back to the 1958 experts' estimate, is probably a statement which can never be proved unless you try to do the research.

I would like to emphasize if you do research it is dangerous to assume that you will necessarily get results.

So I believe that we have taken the correct position in our negotiations of not taking for granted that such research will give results.

On the other hand, I personally—and I am speaking only for myself here—share your concern about the lack of rapid and enthusiastic progress in this field. I believe that this is a situation which is, to some extent, due to the fact that this whole tie-in reaction between science and foreign relations is a very new problem and, therefore, its solution seems to be taking a great deal of time, and this may very well be unavoidable.

Senator HUMPHREY. Thank you very much, Dr. Panofsky.

Go ahead, Senator.

GREATER EFFORT NEEDED

Senator GORE. Doctor, this would require money and men. I, for one, am prepared to vote for both. I know this is not your field, and I will not ask you to reply, but it is necessary that a larger effort be made to perfect systems and mechanisms and means of detection.

I would hope that Mr. Farley could make recommendations in that regard to the proper people in the Bureau of the Budget, the Department of Defense and the Department of State.

So far as Congress is concerned, and I cannot speak except for myself, but I would be prepared to react enthusiastically and favorably in sharing your concern.

Senator HUMPHREY. Senator Hickenlooper wishes to ask some questions.

Senator HICKENLOOPER. Dr. Fisk, this is a question which will, no doubt, call for a speculative answer, but suppose we substantially increased the efficiency of the devices for detection and identification a great deal. Is there any real indication that the Russians would accept that and permit the type and kind of inspection which we would still have to follow for any kind of reasonable assurance against violation of these possible agreements?

SOVIET ATTITUDE TOWARD AGREEMENT AND INSPECTION

Dr. FISK. Well, Senator Hickenlooper, this is one man's answer based on rather limited experience, but it seems to me there is no question but what the Russians, at least, give evidence that they do want an agreement. They have——

Senator HICKENLOOPER. I will go that far with you; they want an agreement.

Dr. FISK. Yes.

Senator HICKENLOOPER. Do they want reliable and adequate inspection?

Dr. FISK. They seem to be a little less enthusiastic about the reliability aspect of the agreement than the agreement itself, I must confess. But they have accepted in principle the concept of inspection.

I do not yet know what this means in practice. They have accepted from Dr. Panofsky's earlier discussions last summer on high altitudes a system here which is quite comprehensive in principle.

I have no reason to believe that ultimately an inspection system that makes sense is negotiable, but I really have no basis for answering your question.

It is a very speculative one. It is the only answer I can make.

Senator HICKENLOOPER. All right.

Senator GORE. Mr. Chairman, may I ask a question?

Senator HUMPHREY. Yes.

DIFFERENCE BETWEEN MISSILE AND WARHEAD TESTING

Senator GORE. Mr. Farley, I ask this question now because I have another appointment.

From the standpoint of disarmament, what distinction would you draw between the tests to perfect an ICBM, which the Russians have just been conducting in the Pacific, and an underground test to perfect the warhead for that ICBM?

Mr. FARLEY. Well, as a problem of inspection, they are quite different.

From the point of view of effect, if there were an arms control agreement relating to one or the other, I think it would depend on the existing state of development in the two arts, the missile delivery art and the warhead art, which the Soviet Union already possessed.

If it already has, as I suspect we must assume, warheads for long-range missiles, and the development that would lie ahead is simply one of improvements, then clearly an agreement which forbids them testing for small improvements is not of as great arms control significance as an agreement which would cut them off before they had attained a significant capability.

I would think before going on it might well—am I at all addressing myself to your question, sir?

Senator GORE. You seem to be approaching it.

Mr. FARLEY. I was not certain I was going to get there before your appointment.

Senator GORE. I have some doubt myself.

I have really been perplexed with the disarmament conference coming up, as to why so much distinction was made between the devel-

opment of a warhead, on the one hand, and an intercontinental ballistic missile, on the other.

It seems to me that both are essential parts of the disarmament problem, and I have some difficulty drawing a sharp distinction between the two if we assume a comparable state of development possessed by each country.

Mr. FARLEY. I agree with that, sir; and I think the answer as to the negotiating priority is really just one of which handle happened to present itself first; for a variety of reasons in terms of our military position and theirs, in terms of world opinion on fallout, and a number of things of this kind, the nuclear test issue became the one which was ready for negotiation on both sides first.

QUESTION OF MORATORIUM ON MISSILE TESTS

Senator GORE. If I ask any question to which you do not wish to make reply, you are perfectly privileged not to make such reply.

I have wondered why our Government has not suggested a moratorium on missile testing in international waters. Had you thought of that?

Mr. FARLEY. In international waters?

It seems to me a somewhat difficult concept to define. I suppose——

Senator GORE. Leave out international waters.

Mr. FARLEY. I suppose as to the impact area then, international waters would come very close, you see, to being an agreement which would cut us out and not the Soviet Union.

I think it is pretty well known that they have only recently begun any missile testing which hit international waters, whereas our past experience has been pretty much directed that way.

Senator GORE. I doubt if that is actually the case.

Mr. FARLEY. I'm sorry if I implied anything on that. I was only saying that they certainly have tested when they did not involve international waters.

Senator GORE. I recognize that.

Mr. FARLEY. So it is a real problem for us.

Senator GORE. Let us omit international waters. The Government has proposed a moratorium on testing of nuclear weapons. Has it considered proposing a moratorium on ICBM tests?

Mr. FARLEY. I think the answer is I believe honestly we have considered almost any possible measure that could be thought of, and in every case there is a great variety of factors which have to be taken into account, of which——

Senator GORE. Then the Government has considered the making of such a proposal?

Mr. FARLEY. And we have not, as of now, thought it a desirable measure to put forward independently.

Senator GORE. But you have considered it?

Mr. FARLEY. Oh, yes.

Senator GORE. You have.

Thank you, Mr. Chairman.

UNITED STATES VERSUS SOVIET ADVANTAGES IN NEGOTIATING

Senator HUMPHREY. Might there be the possibility that the Soviets may be considerably ahead of us in missiles, and we may have a need of doing a little more testing; and that we may be a little bit ahead of them in the sophistication of warheads, and they think they may need a little time to stall to catch up?

I do not want to be too much of a pragmatist or a cynic, but I have been around and from the testimony that one hears, off and on, there is reason to believe, at least we have been told publicly, that our nuclear technology is a superior technology, at least for the moment. From what I have been hearing from my colleagues and about the conflicts within the administration, there appears to be some advance on the part of the Soviets in missiles and rocketry.

We have approached this on a very pragmatic basis. Since they are ahead in rockets, we do not propose any test ban on rockets. Since we appear to be somewhat ahead in nuclear weapons, it surely should not be something that would jeopardize our security to have a test ban in nuclear weapons.

I do not think this is any secret. We have talked about this before. It is the general consensus, I understand, that we do have, in terms of diversity and sophistication of weapons in the nuclear field, an advantage. Is that right?

Mr. FARLEY. I can only give you my personal understanding. I hope this is true.

Senator HUMPHREY. No, not your personal understanding; that is no better than mine. What is the view of the Government on this?

Mr. FARLEY. In order to go into this negotiation we concluded that we would be safe to assume we were as good or better in the nuclear arms.

Senator HUMPHREY. That is exactly what I have been getting to now. Do not misunderstand me; I think that is a good time to negotiate. I do not think it is a very smart thing to negotiate when you are way behind. I have a feeling that maybe the Soviets are somewhat of the same mind, and maybe they are going through a period of some stalling.

KHRUSHCHEV PROPOSAL FOR TOTAL DISARMAMENT

Senator GORE. This raises a very interesting question relating to Mr. Khrushchev's suggestion of total disarmament in four steps.

As I remember, the first step he suggested was in the area in which the United States does, perhaps, and we hope, have superiority. The last step to be reached was the one in which the Communist world has unquestioned preponderance; is that correct?

Mr. FARLEY. It is not my recollection, and I wish I could speak with a little more confidence on this.

Senator GORE. Isn't it?

Mr. FARLEY. I did not believe it began with abolition of nuclear weapons or any similar nuclear measure, if that is what you are referring to.

Senator GORE. What was the first step?

Mr. FARLEY. I'm sorry, but I am almost certain that the nuclear measures were fairly well down the list. This I recall because it struck us as a rather interesting departure.

Senator GORE. I do not have the Khrushchev proposal before me, and I am unprepared to say.

PREREQUISITES FOR NEGOTIATING

Mr. FARLEY. I did want, if I could, just add one comment to the chairman's remark. I have tried to say that relative military posture in any area of arms control is one of the factors taken into account, and I do not think that one should assume from this that there is a necessary equation if one negotiates on what he is ahead in.

You have to look at the balance of all the other elements of your military position also. If you did not believe this, I think we would have to say that we were very foolish to negotiate with anything that the Russians were willing to negotiate on.

Senator HUMPHREY. You balance them off.

Mr. FARLEY. Yes.

Senator HUMPHREY. What I was trying to get at is that there are a number of people who have stated we are in danger of jeopardizing our security in these test ban talks. I had assumed that our Government had weighed this very carefully.

Mr. FARLEY. This is correct.

Senator HUMPHREY. In light of our position in this area of weaponry, as well as other areas of defense, I thought that this was a fruitful area in which to negotiate without jeopardizing the overall balance of strength or balance of forces with the Soviet Union. That is a fair statement, is it not?

Mr. FARLEY. That is correct.

RESEARCH ON CONCEALMENT AND IMPROVEMENT OF DETECTION

Senator HUMPHREY. The other point that, it seems to me, we need to give some attention to is that there are some experiments underway right now. I have here two letters from the Atomic Energy Commission, signed by the General Manager, relating to concealment experimentation. They are not secret. These are open letters.

In my effort to learn the status of certain research projects, I learned that research on concealment is already underway, and preliminary results are already available, but research on improvement is only now being organized.

I have not yet received a detailed response to my inquiry relating to research in the field of improvement of detection from Dr. York. He gave me a very brief interim report.

This emphasizes what I said earlier. The negative type of research on concealment has achieved a higher priority than the positive type of research projects on improvement of detection and inspection. I am going to keep pounding away at this because I think unless we are going to push on the detection and inspection side that we are, in a sense, running some unnecessary risks at a conference, on the one hand; and, secondly, laying ourselves open to some unnecessary criticism.

The Soviets have a way of being able to pick these items up and to do a good propaganda job on us. I do not want to see that happen.

STAGES OF SOVIET PROPOSAL

Senator GORE. In the interest of clarity and accurate information, I have secured a draft study to be issued by the Disarmament Subcommittee entitled "United Nations Action on Disarmament" in which Mr. Khrushchev's proposal is stated in summary form. I would like to read from it, Mr. Farley.

In the first stage proposed by the Soviets, the Soviet Union, the United States, and Communist China would reduce their forces to 1,700,000 each and the United Kingdom and France to 650,000 each. Levels of armaments would be reduced accordingly, and the armed manpower permitted for other states would be decided at a general conference. In the second stage, among other measures, the rest of the armed forces would be disbanded, bases would be eliminated and troops would be withdrawn from territories of foreign states.

It seems to me those two things are hitting at us pretty heavy.

The third step would consist of all other measures necessary to have complete disarmament, including the destruction of all nuclear weapons and missiles, the prohibition of scientific research for military purposes, the termination of all military recruitment and training, and discontinuance of the appropriation of funds for military purposes.

That is the end of the paragraph. That is the summary.

Mr. FARLEY. That omitted only the abolition of the service academies, I believe, which is also in the plan.

Senator GORE. As I listened to the Russian Premier's speech at the United Nations in which he advocated total disarmament, I remembered and found similarity in phrases and language of Mr. Tsarapkin's opening statement at the Geneva Test Ban Conference in which he advocated stoppage of all tests immediately. He said:

We already have a treaty drawn; we are ready to sign today.

Did you see any similarity?

SOVIET NEGOTIATING APPROACH

Mr. FARLEY. I think, sir, that is a familiar Soviet negotiating approach, to emphasize general agreement, agreement in principle, at the outset, and then to work out the details; and, of course, the details can be of crucial importance.

Senator HUMPHREY. In other words, they have the technique of putting the monkey on our back.

Why don't we try that sometime? I never have been able to understand that, because I am sure the Soviets, when they start counting the nuts and bolts and the pennies and the nails, they will be just as difficult and just as meticulous as anybody could be.

But, regrettably, we get ourselves in the position of having to bring up the unfortunate and the unhappy circumstances; and they then, in turn, say, "See, you are trying to block progress toward peace." This is usurpation of the role of peacemaker. Mr. Khrushchev's speech was surely a splendid example of what real propaganda capability and capacity they have.

Dr. Fisk, in the conference, did the Soviets make any legitimate point at all about our data?

Dr. FISK. Do you mean, sir, in challenging it?

Senator HUMPHREY. Yes, in challenging any of the accuracy or any of the deductions from it.

SOVIET PROBE OF U.S. DATA

Dr. FISK. Well, I think I would say this, that in the course of their probing our data, they made us think very hard indeed, and led us to do work that we had not done.

But I think I am safe in saying, and accurate in saying, that they did not find, nor did they point out to us, nor did we know of any conclusions we had drawn which were not sound technically.

Senator, if I may, just for a moment—I understand there was some question as to whether I answered Senator Hickenlooper's question "Yes" or "No."

I would like to, just for the purpose of the record, say that I have no reason to believe that inspection is not negotiable; that is, I think it is probably a negotiable item.

I believe there was some misunderstanding as to whether I said just that or the reverse.

Have I answered your question?

Senator HICKENLOOPER. Well, I may say, Doctor, that you have had a great deal of experience there.

I have been over there at the conferences three times and have followed them rather closely. I wish I could share your optimism.

Dr. FISK. I do not say, sir, this is optimism. I think there is room for progress here, however.

SOVIET ATTITUDE TOWARD ONSITE INSPECTION

Senator HUMPHREY. I believe this is a question outside of the scientific realm. In the negotiations, Mr. Farley, have the Soviets always been willing to accept the principle of onsite inspection from the beginning? They now state that they do accept onsite inspection.

Mr. FARLEY. I believe it would be fair to say that they recognized the principle of onsite inspection even in the 1958 experts' report.

Then, when the political negotiations began, onsite inspection was on their veto list. They subsequently said, provided a quota of inspections could be agreed which was sufficiently small, they would withdraw the veto in the control commission.

Senator HUMPHREY. So there has been some change?

Mr. FARLEY. Yes. But as far as I can recall, they have consistently accepted the principle that onsite inspection might be necessary, and it was part of the control apparatus.

Senator HUMPHREY. Then they went from that position to the veto of the onsite inspection by any type of international team; is that not correct?

Mr. FARLEY. In effect, if the inspections were left to be settled pretty much on a technical basis, any events which did not meet the criteria were eligible for inspection, and the percentage of them were to be actually inspected, then they wanted a veto on the activity.

If it was a flat and small quota, they have said they were willing to forgo the veto.

Senator HICKENLOOPER. Mr. Farley, the Russians announce the principle of world peace and self-determination by peoples, and what they call free elections, and liberty and democracy and freedom and all those things. But it is a question of the details that implement those to a practical result which is where the wheel binds.

IMPLEMENTING THE PRINCIPLE OF ON-SITE INSPECTION

So, of course, they accept the principle. I have heard them practically accept the principle of onsite inspection. But when you get down to how are you going to inspect and what access do you have and what freedom of movement do you have and what liberty do your inspecting teams have and what is their impartiality, that is where they back up. That is where they get into the arguments, according to my experience. So, while they accept the broad principle, just as they announce they are for democracy and freedom and all those things, it is a question of the implementation and the practical operation of the system which is where they keep negotiating ad infinitum to the point of exhaustion and with no results.

Mr. FARLEY. I agree that is the practical test and, clearly, we have not in this negotiation pushed the thing to the point that we know whether there is residually and basically a readiness to carry out their professed position or not.

CHRONOLOGY OF SOVIET POSITIONS

Senator HUMPHREY. I do not disagree basically with the observation made by Senator Hickenlooper, but I want to see if the chronology is right.

In the beginning at the Geneva Conference of Experts, there was at least a tacit or an implicit and maybe an explicit agreement that there would be the necessity for onsite inspections.

Dr. FISK. Explicit.

Senator HUMPHREY. There was not, spelled out in terms of the composition of the team to do it, such agreement?

Dr. FISK. Quite right.

Senator HUMPHREY. Subsequently, there was an argument in the political conferences and the political negotiations over the composition of any kind of international inspection teams.

Mr. FARLEY. I would say, Senator, that prior to that came the dispute as to the procedure for selecting the events——

Senator HUMPHREY. Right.

Mr. FARLEY (continuing). And for dispatching the teams.

Senator HUMPHREY. Then there was the argument over the composition of the teams?

Mr. FARLEY. That is correct.

Senator HUMPHREY. Later on there was at least some indication of a minimal number of onsite inspections—a quota that would be acceptable by a team over which a veto was not exercised.

Mr. FARLEY. Correct.

Senator HUMPHREY. Is that correct?

Mr. FARLEY. That is correct.

INDICATION OF SOVIET ACCEPTANCE OF FEW DOZEN INSPECTIONS

Senator HUMPHREY. And in the final report—or if not in the report, possibly it was in the discussions on the report of the second technical conference—the chairman of the Soviet delegation indicated that once the criteria were established there would, perhaps, be a few dozen onsite inspections at the discretion of one of the parties.

What I am getting at is that stage when the Soviets came around to recognizing more than two or three. The chairman said a few dozen onsite inspections.

Dr. Fisk or Dr. Panofsky, does this mean that the Soviet technicians recognized again the necessity of some onsite inspections?

Dr. FISK. Senator, that particular statement did not——

Senator HUMPHREY. This is in the political conference; that is right.

Dr. FISK. That happened in the political conference.

Senator HUMPHREY. That is correct.

Dr. FISK. But I think—I cannot answer the question directly—but the first time in which, to my knowledge, the Soviets ever accepted the concept of onsite inspection was in the 1958 conference in Geneva, and this was indeed a technical conference.

Senator HUMPHREY. Yes, sir.

Dr. FISK. So I have no reason to believe they feel any differently today—no evidence.

FIRST SOVIET POSITION ON INSPECTIONS

Senator HICKENLOOPER. But did not they insist in the first part of these negotiations that this was basically a political matter, and that a treaty agreeing to the cessation of all tests was to be signed first, and then any details on onsite inspection would later be worked out by another group disassociated from the treaty itself. Perhaps you were not associated with it.

Dr. FISK. I was not associated with it. That is in the political conference.

Mr. FARLEY. That is correct; that was their first position.

Senator HICKENLOOPER. From the acceptance of the concept of onsite inspection, they waved that away and said, "Of course, inspection is all right, but first we have to sign a treaty outlawing all tests, and then we will proceed to negotiate at a later date on these technical matters as to how we are going to inspect and what the program is. We will not mix up the scientific matters with the political negotiation of a treaty which is purely political."

That was the attitude I think they took. I think it has become intermingled more or less. A year and a half has gone on by absorption or confusion or by some other method, but I think that is the way they started out.

Senator HUMPHREY. Mr. Farley, what do you think the Soviet position is today on onsite inspections?

I agree with Senator Hickenlooper in reference to the original presentation of the Soviets relating to the treaty, which was "accept this, No. 1; No. 2, we will work out the Geneva details."

They wanted to accept the original Geneva Conference of Experts in toto, and then work out the details. What do you say it is now?

SOVIET ASSESSMENT OF INSPECTION REQUIREMENT

Mr. FARLEY. Well, first, I believe that one never knows the Soviet position or the position of anyone with whom he is negotiating until you have actually finished an agreement and gotten all the details down. We certainly have not done that.

It is clear to me that the Soviet Union has assessed the reasonableness of the requirement for onsite inspection in the context of a test suspension agreement; has concluded this is a reasonable requirement which it would have to accept in order to maintain its posture of wanting an agreement.

It is clear also that its concept of the number of inspections it would like to see in the Soviet Union, with all its record of insulation from the outside world, is pretty far from the number that has appeared to us necessary, and, at present, it seems very doubtful that the two are going to be brought together.

Certainly, the record of this technical discussion which Dr. Fisk and Dr. Panofsky are reporting on is not encouraging in this respect, since one who reads the report as a whole, this seems to be the core of the disagreement. That is the level of inspection that will finally emerge from all these considerations of criteria and seismic signals, and so forth.

But it does seem to me that as the Soviet Union presents its position it has conceded the reasonableness of the requirement for onsite inspection, and is concerned primarily with the quantitative side.

PROGRESS NOTED ON ACCESS TO AREAS

Now, there remains also one other point on which negotiations have been only partial so far, and that is the details of the procedure to insure timely and thorough access to areas.

While those have not been completed, the progress has been fairly good, and it does appear possible that that aspect would be negotiable.

LANGUAGE AND BEHAVIOR OF SOVIET SCIENTISTS

Senator HUMPHREY. I have just one or two more questions.

Dr. Fisk or Dr. Panofsky, there is great talk about the political attitude of the Russian scientists, and some people went so far as to say that the Soviet scientists furnished not a scintilla of scientific information at the recent Conference of Experts.

I would be interested in your views as to the language and the behavior of the Soviet scientists. Did they demonstrate any scientific objectivity? Was their language intemperate? Did they bring a scintilla of scientific information? What was the situation?

Dr. FISK. Well, sir; a scintilla is not a unit of measure that I am familiar with.

Senator HUMPHREY. I am quoting a Commissioner of the Atomic Energy Commission; I am not familiar with it. Mr. Floberg said that.

Senator HICKENLOOPER. It is a legal term, Doctor, and it is quite nebulous in its definition, I will admit.

Dr. FISK. Let me say this: They were, until the last day in which we reported to the political conference, they were their usual selves.

On that last day they said some things I thought were very intemperate, regarding the integrity of our people and of our work.

EVALUATION OF SOVIET CONTRIBUTIONS

On the whole, when we could get them to discuss the important technical issues, they discussed them sensibly, and they did make contributions. They made us think very hard, indeed.

On the other hand, their tactics appeared to us to be one of avoiding discussing the new Hardtack data, avoiding discussing anything that might make the system look less good than it had previously appeared.

They did this largely by bringing in rather legalistic, formalistic kinds of arguments. But I would say that when we got to technical things they talked sensibly, and they are, indeed, a very able group of people.

Senator HICKENLOOPER. Mr. Chairman, may I just pursue that for a moment?

Senator HUMPHREY. Yes.

Senator HICKENLOOPER. You said that when they got to discussing technical matters, Dr. Fisk, that periodically they made contributions in the discussion.

How do you define the contributions? In other words, did they contribute data or information which they had developed, or did they merely make a contribution by a reasonably intelligent discussion of already existing knowledge?

Dr. FISK. Well, some of both, Senator.

Senator HICKENLOOPER. In other words, could you say that it was an original contribution and the furnishing of any new data or any verifying facts which they may have developed themselves, which would be a real contribution rather than a contribution which comes from intelligent social discussion or scientific discussion of a known or existing situation or fact?

Dr. FISK. Well, I would say that in this particular Conference, the technical work in Group No. 2, that their direct contribution in terms of new data, measurements, studies, was quite minimal, quite small.

Their contribution was——

Senator HICKENLOOPER. Did they get down to the points of a scintilla?

Dr. FISK. A semiscintilla.

Senator HICKENLOOPER. Well, that is pretty small.

It approaches deminimis.

Dr. FISK. But their critical comment, critical in a good sense, of our own analyses, and so on, was good.

However, I should add that on our previous meeting, and I believe it is also true at Dr. Panofsky's meeting, am I right in saying, they made contributions to it.

Dr. PANOFSKY. Yes, I will comment on it. I think it is difficult to measure their contributions in a simple way by considering the sit-

uation in which their scientists find themselves, and in which our scientists find themselves to be symmetrical.

MOST TECHNICAL CONTRIBUTIONS CAME FROM WEST

I think it is true that in all of the three technical negotiations, by far the largest part of the technial contributions had been made by the U.S. delegations, and the reason for this, I think, is the fact that the onus of proving the workability of a control system had always rested on the Western delegations because of the fact that as far as the Russians are concerned, these negotiations are basically a political negotiation in which the trade off is being made in terms of wishing to achieve cessation of nuclear tests versus some concessions, but not more than necessary in terms of their privacy of their country, and the question of detailed technical performance of the control system is our lookout.

It is for that reason, I think, that the general context of these negotiations is such that it was automatic that the majority of the technical contributions will come from the Western, and particularly, the U.S. delegations. I think it is unavoidable.

NATURE OF SOVIET CONTRIBUTIONS

I think there have been substantial contributions of a theoretical nature; there were some seismic data of our explosions, and miscellaneous other material was contributed by the Russians.

Their contributions, however, in all three of these negotiations were only, I believe, for the reasons indicated, a relatively small fraction.

Senator HICKENLOOPER. Well, Dr. Panofsky, at the outset the data were produced by the United States, based first on the Rainier and then on the Hardtack shots.

We furnished the technical data and any scientific conclusions or any scientific activities that occurred in connection with them. We let those out; so I would assume that was a contribution at this conference from a scientific standpoint.

Now, did the Russians contribute any data which they may have accumulated as a result of their alleged chemical explosion experiments?

Dr. PANOFSKY. Of course, we have no way of telling what they are contributing and what they are holding back. But they have contributed some description and data from some chemical explosions. They did put them on the record.

Senator HICKENLOOPER. I see.

Dr. PANOFSKY. They did put on record seismic records taken from our explosions, taken at very large distances.

They did put on the record certain elaborate theoretical contributions having to do with the way in which seismic waves would be distorted by propagation through the earth's crust.

So they have made contributions. But we do not know, of course, whether they have information bearing on these matters which they did not introduce. We have no way of telling.

Senator HICKENLOOPER. I see. Thank you.

FIXED CRITERIA TO LIMIT INSPECTIONS

Senator HUMPHREY. Is it a fact, or is it your observation, Doctor, that having once accepted onsite inspection in principle and then in some detail, that what the Soviets are attempting to do is to establish fixed criteria which will limit the number of onsite inspections? They have come to recognize that any kind of workable inspection system that we would ever accept or about which there could ever be any hope of acceptance, would have to include onsite inspection and, therefore, what they are arguing about in the political areas is the ascertainment of fixed criteria to limit the number of onsite inspections so that the secrecy, which has been characteristic of the Soviet territory and Soviet system, will not be, as they would put it, violated too much.

They constantly talk about groups roaming around their country spying on them. They have this guilt complex which comes from long habit and experience of worrying that the inspection teams will not be inspecting for suspivious events, but rather for other things.

Would you say that is the general feeling?

Dr. PANOFSKY. I think that is correct. They have accepted the principle, and they are trying to use both the political conference and the last scientific conference as a lever to minimize invasion of their privacy by the inspection teams.

BASIC SOURCE OF DISAGREEMENT

It is this latter part; namely, trying to use the scientific conference as a means to this end, which is really the basic source of our disagreements.

Senator HUMPHREY. Yes.

Dr. PANOFSKY. Namely, that they adopted the axiom that it is the Soviet mandate to write rules to minimize the number of inspections, and we took the attitude that we cannot write these rules within present knowledge with safety.

Senator HUMPHREY. This was my feeling about it. In the earlier political conferences the Soviet delegates possibly went a little further than had been anticipated originally in terms of onsite inspection acceptance. Then the scientists were literally ordered to try to develop criteria which, on the one hand, would preserve the principle of onsite inspection since it had been enunciated several times by the Soviets, but, at the same time, would actually limit drastically the number of onsite inspections, the procedure to conduct such inspections, the areas to be inspected, and a host of other factors. That has been my feeling about it.

Just this one question, and Senator Gore wants to ask you a question on a matter going back to a previous question.

IMPORTANCE OF CALCULATING MAGNITUDE OF EXPLOSIONS

There was some discussion about the magnitude of nuclear explosions, the calculating of the magnitude of the nuclear explosions.

Can you tell us anything about why this calculation of magnitude is important? Was there any contribution made here by the Soviets or any interrogation of the Soviets in discussion?

Dr. FISK. I think, Mr. Chairman, this is one that Dr. Romney should take on. My own view is that the Soviet contribution in this particular area was negative.

Dr. ROMNEY. Well, the importance of the question is that the existing statistics on numbers of earthquakes which occur throughout the world are compiled in terms of magnitude. So that to make an estimate of the number of natural events which the control system will have to cope with, you must somehow relate the signal size from nuclear explosion to the signal size from an earthquake.

SOVIET CRITICISMS OF U.S. CALCULATIONS

The Soviet criticisms of our procedures, in my view, are dishonest.

They took the point of view that we should reject all data which tended to indicate there would be more earthquakes. In other words, they said let us forget about all the low determinations of magnitude, and consider only the high ones. They followed this procedure with as many technical and pseudotechnical arguments as they could through the whole proceeding.

Their final paper, their final report, contains information which, I believe, is incorrect and which I believe they know is incorrect.

SOVIET CALCULATIONS REDUCE NUMBER OF INSPECTIONS

They have computed the numbers of earthquakes or the size of earthquakes on one magnitude scale, and the size of explosions on another magnitude scale.

This is kind of like measuring the height of a man in feet, and then saying he is 6 yards tall. The result is to make the numbers of inspections appear smaller, and I think clearly this was their objective.

Senator HUMPHREY. This all goes back to the same point again. Since the Soviets reluctantly accepted the principle of onsite inspection, they have to trim it down, they have to circumscribe it. They feel compelled to limit it to a point where its effectiveness or its usefulness is sorely affected. Isn't that about it?

Dr. ROMNEY. Yes.

UNMANNED CONTROL POSTS

Senator HUMPHREY. Why didn't you recommend the inclusion of unmanned stations, Dr. Fisk or Dr. Panofsky, or did you? The Berkner report said that you could improve detection by increasing the number of manned control posts or by adding unmanned posts.

Did you make any such recommendation to this second conference?

Dr. FISK. Not explicitly, Mr. Chairman.

We did by implication refer to the parts of the Berkner report and things that we had not discussed in detail, but which were there and which they knew very well what we were referring to.

Secondly, they know as well as we that the efficiency of a system, whether manned or unmanned, depends on the number of stations, the number of instruments, if you like.

But more importantly perhaps than either of these two reasons for not having brought it out was that there are very many questions of feasibility, that go beyond feasibility.

We know it is technically feasible; however, the security of the system, the engineering estimates, the economics, the political implications, and so on, which we felt would have to be studied through before we could make a positive presentation to say we must consider this, had simply not been done at the time we were in Geneva nor were we in a position to do it in the short time we had there with all the pressures.

Senator HUMPHREY. And your failure to bring it up is no indication and should not be interpreted as any indication that you do not consider the unmanned station concept worthy of study.

Dr. FISK. No, by no means.

It seems to me it is something that ought to be looked into very thoroughly and very carefully.

Senator HUMPHREY. Senator Gore?

KHRUSHCHEV'S SPEECH BEFORE U.N.

Senator GORE. Mr. Chairman, it was an error for me to make a reference to the speech of the Russian Premier without having refreshed my memory and without having forewarned Mr. Farley.

I have been reading further, however, and I find that my recollections of it is somewhat confirmed by this. In his speech before the General Assembly Mr. Khrushchev stated that—

> Since negotiation of a comprehensive disarmament agreement would take time, the test suspension agreement should be signed first.

And then there is this:

> In addition Mr. Khrushchev said that if the Western Powers were not ready to embark on the program of general disarmament which he suggested, the Soviet Union was ready to agree on other partial steps toward disarmament. The examples of partial steps he gave primarily concerned Europe. One, the creation of a zone of control and troop reduction in Europe—

which I must presume would include Berlin.

> Two. The creation of an atom-free zone in Central Europe.
> Three. The withdrawal of foreign troops on European territory.
> Four. A nonaggression pact by members of NATO and the Warsaw Pact.

I apologize to you for asking you about it without having forewarned you, and for having referred to it myself without freshing my memory.

WITNESSES' VIEWS ON WHETHER UNITED STATES SHOULD PURSUE TEST BAN NEGOTIATIONS

Senator HUMPHREY. Dr. Fisk, do you think that we should pursue these negotiations?

Do you think there is any reason to believe that by diligence and perseverance and constant reexamination of the data which are made available through scientific research there is any hope of arriving at an agreement between ourselves and the Soviet Union and other parties?

Dr. FISK. I have such hope, Mr. Chairman.

Senator HUMPHREY. Is this a hope that comes from your kindly and optimistic nature or does this come from your scientific observations?

Dr. FISK. More from the latter. I have little of the former, sir.

Senator HUMPHREY. You have, thank goodness, a great deal of the former, namely, the kindly and optimistic nature, which is one of your great qualities, my friend.

I respect your scientific knowledge, too, and your judgment.

Is it your view from your observations and your judgment that this discussion or negotiation should be pursued and, if so, that there is a prospect of success?

Dr. FISK. Well, sir, it certainly will not be easy. It will not be quick. But I, for one, would by no means give up hope that we can come to a sensible conclusion.

Senator HUMPHREY. Dr. Panofsky, what is your view?

Dr. PANOFSKY. Again I would only give my highly personal opinion. I believe that it is extremely essential to pursue these negotiations, I think, for two reasons:

First, the negotiations are essentially a test case, so to speak, on this really very difficult problem, namely, the genuine desire for disarmament, the desire of the Russians for secrecy, and our desire for control. Just exploring what areas of agreement can be achieved between these three items, is extremely essential.

But, even if the concept of a comprehensive ban has to disappear in the process, a question which should be explored to the fullest, I think that by pursuing these negotiations we cannot help but be wiser in future ones, because clearly the problems which arise here will, by necessity, arise under different names in future negotiations.

Senator HUMPHREY. Dr. Romney, what is your view? You have heard my question.

Dr. ROMNEY. Yes, sir. I do not really feel I have thought enough about the matter in full context to have an intelligent judgment.

I would like to see a truly controlled test ban, and hope that we continue to negotiate until we find we can or we cannot.

IMPROVING DETECTION CAPABILITIES

Senator HUMPHREY. Do you think, sir, in light of your knowledge of seismology, that we can improve this science or this technique to a point where it will satisfy the requirements of an effective and safeguarded system of inspection and control?

Dr. ROMNEY. I will guarantee we can improve our capabilities. I think the political problems, however, are greater than the technical. I really could not answer fully.

Senator HUMPHREY. Do you think you can improve your capability to the point where you, as a scientist, could recommend to a political officer that our security would not be jeopardized by an agreement?

Dr. ROMNEY. I am sure I could not say that purely from the standpoint of seismology. There would have to be many other considerations, particularly of the onsite inspection techniques, that would enter into that.

Senator HUMPHREY. Mr. Farley, do you have any general observation you would like to make?

Mr. FARLEY. I think, Senator, it is hard for me to do much more on this than to refer to the answer the President gave yesterday that we are going to pursue these as long as we see any prospect of getting agreement.

I would like to make one comment on the little triad that I believe Dr. Panofsky referred to, because you said one thing earlier that rankled a bit, and that was the implication, I thought, that in raising these problems of control we are taking a negative position.

I believe that, in general, we have succeeded in the past few years in the discussions of disarmament in the United Nations and General Assembly in maintaining the position that inspection and control is a real assurance to both sides in any arms control agreement.

We certainly have our present arms establishment, not for any aggressive purposes but for the purposes of protecting ourselves and our friends, and for the purposes of assuring world peace.

If we give up the present establishment or parts of it or the possibilities of improving it, as we could by nuclear test suspension, as we would in a nuclear test suspension, it is important, I think, not just in a negative way but in a positive way that there be assurances that an agreement is carried out so that both sides and all the world can be certain that the world peace is not being risked in the course of such an agreement.

I do not mean to imply that you meant anything else.

Senator HUMPHREY. I understand.

Mr. FARLEY. But I was afraid the record might appear as if somehow we, in raising these problems—and there are problems—were raising obstacles rather than necessary elements of any agreement that is to stand the test of actually improving world peace.

NEED TO EMPHASIZE SOLUTIONS TO TECHNICAL PROBLEMS

Senator HUMPHREY. No. I think your point is well taken. I was attempting to point out that the problems that are great and perplexing seem to get the public attention, and this attention is seized upon by the Soviet propagandists and interpreted——

Mr. FARLEY. That is very true.

Senator HUMPHREY (continuing). Interpreted as a sign that we really do not want an agreement.

What I was concerned about was getting a little more emphasis upon the answers to the problems.

Mr. FARLEY. That is very correct.

Senator HUMPHREY. Now, I realize that it is easier to criticize than it is to correct. But frequentlly we are reminded that if we are going to spend our time criticizing a program or pointing out the inadequacies of an administration or of a project or a policy, we have some responsibility to point up our solutions. I happen to think it would be good for us to really let the world know, as well as our own general public and our own community of scientists, that we are making giant strides or at least making great efforts to improve in the field of research on these matters of detection and inspection.

INTRODUCTION OF DR. TUKEY

Dr. Tukey, I think we have let you off far too easy. I have had a blinder on here. I apologize, first of all, for what might be interpreted as a lack of appreciation of your attendance and, secondly, I apologize for not having dipped into your mind today to get some good information.

You go right ahead.

STATEMENT OF DR. JOHN W. TUKEY, MEMBER OF U.S. DELEGATION TO TECHNICAL WORKING GROUP 2, GENEVA TEST BAN CONFERENCE

Dr. TUKEY. I think I am just here at your service and will be glad to try to answer anything that you try to ask.

Senator HUMPHREY. May I ask the general question that I put to the other gentlemen? Do you believe that, No. 1, it is in our national interest to pursue these negotiations; secondly, do you believe that we could from a technical point of view arrive at an agreement that would be relatively safeguarded in terms of inspection?

IMPORTANCE OF PURSUING NEGOTIATIONS

Dr. TUKEY. Well, let me take these in order, if I may. My own feeling would be that it is entirely essential to continue the negotiations for both the main reasons that have been cited here earlier: First, that there is a real hope that in this particular instance we might come to a successful conclusion, and even if it takes a long time, it would still be worth the effort.

Secondly, we do have this problem of exploration, of getting into the middle between the three forces that Professor Panofsky mentioned and trying to find out by what methods these can be resolved.

NEGOTIATIONS ARE A TEST PROBLEM

I cannot see how there could be a better test problem in which to learn about methods of resolution here. This problem is very much simpler than most of the other assigned problems.

Senator GORE. You said simpler?

Dr. TUKEY. Simpler.

Mr. FARLEY. It is a discouraging thought.

Senator HUMPHREY. I do not think it is discouraging when you think of what would happen if you had a 15-megaton bomb hit you. I think that is what you ought to start out with every day and ask yourself:

How would you like to be on the fringe edge of one of these bombs, one that did not put you out of business, but just let you lie around for 50 years.

Dr. TUKEY. Let me tell you a little bit about why I think this one is simpler.

The technical facts here are mainly the technical facts of physics and seismology, information processing, and things of this sort.

These are better understood and more discussed on an international level than the technical problems of military events.

We have a much better understanding among a larger public, I think, of these technical problems. We can get more help from them.

TRAINING OF SEISMOLOGISTS

You were raising this question of could we train enough seismologists. I think it is much easier to train seismologists than to train military specialists. We have more people who are used to training in this sort of thing. We have more people coming up who would be more interested in training.

So I really think this is one of the simple problems. It is obviously a very difficult one, but by comparison it is simple and, therefore, it is a good one in which to explore methods.

A CONTROL SYSTEM TECHNICALLY POSSIBLE

Secondly, you asked about the possibilities of improvement. I would like to first reiterate one of the things that was said earlier, and that is that within the present purely scientific capabilities, if there were no limitation on the number of places at which control equipment were placed to detect explosions, it is obviously technically possible to provide a control system.

The difficulty is to find technical means that will be compatible with what can be politically negotiated. So in one trivial way I would answer "Yes" to your question.

In a less trivial way I would certainly completely agree with what Dr. Romney said; namely, that improvements in seismology can be promised. The precise extent of the improvements and the dates when they can be delivered are not things that can be promised.

This is a matter of research and not of development or of routine engineering. But the technical capabilities are going to improve.

The question still is will the technical capabilities come to meet the political realities, and this is not a purely technical question, and neither the technical nor the political side depend on things that are known to anyone at this particular hour on this particular day.

But I think I have a feeling of honest optimism that it is worth serious effort in this area not only in negotiation but also, as you were pointing out earlier, in research, to provide improved capability.

STATEMENT OF DR. KISTIAKOWSKY ON CONTRIBUTION OF SCIENTISTS

Senator HUMPHREY. Dr. Tukey, a statement by Dr. George Kistiakowsky in a recent speech, was just brought to my attention. He said:

> The success of the future negotiations to relieve tensions by arms limitations agreements will depend in some measure on the understanding of the capabilities of the proposed multilateral monitoring systems and the understanding of the inherent limitations of any monitoring system in a world of rapidly advancing technology. The limitations of technical analysis need also to be fully understood. There is no doubt, for instance, that the reliability of monitoring systems is largely a technical question. But the adequacy of such systems from the point of view of national security is not. It is a political-military question. So deterrence is not a scientific concept but a political-military one, and we must realize that political issues or disagreements cannot be resolved by technical agreement on facts.

The political questions of national interest remain. I think it is well that these issues be understood by the public as they are being understood by the policymakers. Especially we, as scientists, must understand we can contribute one of several inputs that are essential for the formulation of sound national policy.

I gather that this is a rather succinct analysis of what has been stated by you gentlemen in terms of the contribution that the technician and the scientists can make, and that sometimes there are overriding national interests which compel one to set aside the technical aspects or to alter their meaning. Is that something which would meet with your view?

Dr. TUKEY. I think I would certainly agree with what Dr. Kistiakowsky had to say.

TECHNICAL FACTS AS A BASIS FOR POLITICAL DECISIONS

I think I would phrase what he had to say just slightly differently. It is not a question of altering or setting aside the technical facts; it is a question of using them as one of the foundations for the political decisions.

Senator HUMPHREY. Very good.

Would you care to ask any questions?

Senator GORE. Mr. Farley, I am wondering how this conference and negotiation relates to the disarmament conference to which, I believe, our Government has agreed, and which is expected to begin in Geneva next month?

From all the testimony we have heard here there is little room for optimism, it appears to me, for an early agreement on cessation of all tests with adequate inspection, particularly since we have not been able to reach any agreement on the technical aspects of detection; and I believe no further conference of scientific experts has yet been arranged.

Mr. Khrushchev has said that since negotiation of a comprehensive disarmament agreement would take time, the first step should be to sign a nuclear weapons test stoppage treaty. How does this relate to the general disarmament conference that is soon to be underway?

RELATIONSHIP BETWEEN TEST BAN NEGOTIATIONS AND 10-NATION DISARMAMENT CONFERENCE

Mr. FARLEY. The present negotiation relates to one small part of the area of disarmament, and it is true that if this negotiation were to reach some agreement, one thing that might be done would be to either take note of the thing or incorporate it in broader negotiations toward a broader agreement.

Since the prospects, I agree, are not encouraging that there will be substantial agreement in the nuclear test negotiations, one is, I think, asking what the effect of the disagreement will be on the broader negotiations. Certainly, it starts them under something of a cloud.

On the other hand, I think the issue has always been in the general disarmament field, as well as the nuclear test field, whether we can negotiate an equitable agreement which provides for adequate safeguards and assurances, and I would take the present nuclear test suspension negotiations as, in effect, the opening test case or skirmish,

whatever metaphor you want to use, in what inevitably is going to be a very long give and take.

I do not think any of us expect comprehensive general disarmament to be negotiated in a few weeks. Progress that can be made is going to be painful and time consuming.

We are certainly going to put our best efforts on it. But among the things which must be tested is the possibility of getting an agreement on an adequately inspected and controlled basis.

So that what we have seen in these test suspension negotiations will be an initial exploring of the approach of both sides, a test of willingness to face the technical and other facts on which agreement must be shaped, a kind of probing of whether there is weakness in the position of either side.

TEST BAN TALKS, A REHEARSAL FOR DISARMAMENT CONFERENCE

In many respects this is a rehearsal or test for what will happen in the general negotiations, and I think it probably will have its chief significance in that respect because, as we have both recognized, the chances that there will be an agreement consummated by about March 15 are not good.

Senator GORE. Will this conference on a specific item of disarmament, which you say is but a small part of the total problem of disarmament, continue as the other conference is underway?

Mr. FARLEY. It could well do so.

There is an important difference. This negotiation is related to a specific agreement where we are actually working article by article on a treaty, and its annexes.

The general disarmament negotiations, at least initially, would, I am sure, be more exploratory. They will be more an exchange of views, a table of positions, an effort to identify areas of common ground and areas of disagreement, rather than a specific treaty or other kind of an agreement; so that there will be a difference in approach in the general nature of the meeting.

Senator HUMPHREY. Anything else?

Senator GORE. Yes, please, Mr. Chairman.

Are you saying, in essence, that this has been a long and determined effort to probe the possibilities of bringing the Soviet Union into an acceptable agreement of disarmament with adequate inspection and that this is a specific instance of that?

Mr. FARLEY. This has been an explicit objective on our part, Senator.

NATURE AND EXTENT OF TEST BAN PROBLEMS

I would want to comment on what Dr. Tukey said. While it is true that perhaps this problem is one which technically is simpler than some others, from the point of view of negotiating an international agreement we have had all the problems, I think, that one could have in negotiating a comprehensive agreement.

We have had to face everything from the details of privileges and immunities of inspectors, but everything, so that in scope in that respect this is not something which has been a minor negotiation.

So that in two respects then this has been a very important trial case.

It has been the kind of test you mentioned of the Soviet readiness to negotiate a sound, safeguarded agreement, which is very important for determining their intentions and their negotiating approach to broader negotiations.

But it has also been a very important and thorough pilot case of an international agreement providing for international administration of quite elaborate controls, and that has required a great deal of pioneering, legal, international organization work.

SPECIFIC RELATIONSHIP BETWEEN NEGOTIATIONS

Senator GORE. So the relationship of this negotiation and the broader and more general negotiation is very specific?

Mr. FARLEY. Oh, yes. For example, if one were to meet with success in the general negotiations, and identify areas where we could proceed to specific negotiation, one might well relate the control organization of the two, as there are many aspects of this system which are specific, the seismic detection stations and the air sampling flights, this sort of thing.

But there are other aspects of the administrative machinery which might have to be brought into relationship with another control organization that might provide the framework for it.

So this could be a building stone on a larger building.

ADMINISTRATION'S ATTITUDE TOWARD DISARMAMENT CONFERENCE

Senator GORE. The administration, I take it, then, is prepared to proceed with the general negotiation in good faith and in earnestness, despite the discouragement of the specific effort now underway?

Mr. FARLEY. That is correct. This arises from two things:

One, our determination to do everything we can to initiate progress toward the broader disarmament goals of the country, I think, not just the administration.

Secondly, I think there are real areas of agreement even if they are only partial, which are merged in this negotiation which make it not simply idle or futile to go into general negotiations with some hopes that we will make some progress.

Senator GORE. These are some of the reasons, Mr. Farley, why I had suggested several months ago, and have since repeated, in all earnestness, the desirability of making a start in this specific field in the particular areas and spheres in which detection was not as complicated as the detection of underground tests.

IMPORTANCE OF ACHIEVING SOME SUCCESS IN TEST BAN NEGOTIATIONS

I do not wish to have an attitude of saying, I told you so, or wish to say anything further about that, but I regard it as of utmost importance that some success be achieved in this field, perhaps not before the very beginning of the general disarmament conference, but in the early stages of it. If the President's position of last April 13 could be achieved, or if even the more narrow objective of my suggestion of November 1958 could be accomplished, it would represent some step forward which might serve as an encouragement for the broader goals of general disarmament.

Mr. FARLEY. I think these are important observations and I agree with them.

Senator GORE. Thank you, sir.

Senator HUMPHREY. Mr. Farley, at a later date we are going to see you again. We have a number of hearings coming up, and there are many problems.

"LEAK" OF U.S. TEST BAN PROPOSAL

I am deeply interested in the allegations that were made in the column by Mr. Childs the other day, which I mentioned on the floor of the Senate, and in the fact that the proposal which our Government was going to advance became public knowledge before it ever became private knowledge, or at least before it was privately discussed with the other delegates.

How that happened, I do not know, and I think the Department of State ought to find out how it happened. I think it is a poor way to negotiate anything, if you want to negotiate, and I gather you do.

NEED TO DISCUSS INCLUDING RED CHINA IN A DISARMAMENT AGREEMENT

The second point is that I would like to call to your attention that we had better discuss how we are going to get China into a disarmament agreement. I gather now that everybody recognizes what took place in this room 4 years ago when the late Secretary of State, Mr. Dulles, was here. I asked him whether he agreed with me that you had to have China included within a disarmament agreement or nuclear weapons test suspension agreement in order to have any reliance in the agreement. You can examine the record as to what was said, but the gist of it was: well, we have not come to that yet. It would be better if we did not talk about it right now.

We are going to have to talk about it, and we might just as well face up to it.

It is not going to be easy, so we had better talk about how to do it, so I want to alert you because I am going to ask a lot of questions about this.

Senator Gore and Senator Church and others have discussed this many times in the Senate, so I am sure we will want to go into that.

IMPORTANCE OF HALTING THE ARMS RACE

My final point with you today, gentlemen, is while I know all of these matters are difficult, and I surely would tend to become somewhat pessimistic if I did not face the other alternative, the maddening stepped-up pace of the arms race. I have become convinced that unless we can do something to bring this arms race to a halt, we are going to deny ourselves the privilege of bringing it to a halt. It will halt us.

In other words, it can get out of hand. This is something that is not given much attention these days because we are more interested in closing gaps than we are in preventing the world from becoming a gap, a big one, one big Grand Canyon, with just about as much life and as much vegetation as is to be found at least down about halfway or around the upper rim, I should say, of the canyon.

I am concerned over the prospects of an uncontrolled arms race, and I think that every time we negotiate we ought to think of the other alternative. The other alternative is to face up to a disciplined, dictatorial, monolithic society that has priorities, that will engage in an arms race at the sacrifice of its own people for a long period of time, when, in fact, it is questionable whether we, as a people in a democracy, would want to continue an arms race over a long period of time. And if we did, it is highly questionable whether the democracy that we love and the standard of living that we so cherish could be sustained.

THE SPREAD OF NUCLEAR WEAPONS TECHNOLOGY

But more importantly it might get out of hand. I worry about the day that those Chinese Communists get hold of one of these atom bombs that they can make themselves, not one that is spoonfed to them by Mr. Khrushchev. I think Mr. Khrushchev would rather run the world than see Chou En-lai run it.

What I worry about is the day that this technology does spread, and more and more of you men of science are telling us that it could spread, and it could get into their hands in terms of an effective nuclear weapon. When that day comes, it will not be a matter of arguing with Mr. de Gaulle or anybody else whether they should have a test in the Sahara Desert. They will have one, and then we will be in trouble.

SHARING NUCLEAR WEAPONS WITH ALLIES

Senator GORE. Won't these questions have to be considered in connection with legislation, if requested, to which the President made reference yesterday?

Senator HUMPHREY. Indeed they will. I am not at all happy with the President's request of yesterday. It is bad enough for three countries to have weapons in their possession, and as much as I love our allies, I am not sure of all of their colonels who may be in charge of these weapons. I am not sure that prime ministers that you can trust will always have the final word. I may be wrong. That is the concern I have.

IMPORTANCE OF WORK OF DISARMAMENT SUBCOMMITTEE

One of the reasons I feel that the work we do on this subcommittee is worthwhile is not because of the very limited results we obtain but because somewhere in this Government we have to be talking about how we can find a way out of this organized insanity and madness of spending ourselves, as a people, into ever-increasing indebtedness, raising the cost, and bringing onto ourselves the possibility of extermination, and settling no problems.

ESTABLISHING AN ASSISTANT SECRETARY OF STATE FOR DISARMAMENT

I would, therefore, suggest that we ought to upgrade the field of disarmament away from just happenstance talk.

One of these days in the near future I am going to introduce a bill, Mr. Farley, to establish an Assistant Secretary of State in the field

of disarmament. I am also going to introduce some other legislation because it is one thing to have the State Department and another thing to have the Defense Department, but I want to see a country that is supposed to be dedicated and is in its heart dedicated to peace, put it right up there in capital letters spelling it out P-E-A-C-E with the full knowledge of what we mean, not appeasement, not running away from the problems, not avoiding them, but the kind of peace that requires more courage than even the waging of a war.

With that, I would like to adjourn the meeting, unless there is somebody else who wants to say anything.

ACCOMPLISHMENTS OF TEST BAN NEGOTIATIONS

We will include in the transcript the list of accomplishments thus far of the Conference on the Discontinuance of Nuclear Weapons Tests, the items agreed to, to which you referred, Mr. Farley, and the items not agreed to, to which you have all referred.

(The text of the documents referred to is given in the appendix, starting at p. 84.)

Senator HUMPHREY. I thank you all very, very much, gentlemen.

(Whereupon, at 6:15 p.m., the subcommittee adjourned.)

APPENDIX

LETTER FROM SENATOR HUMPHREY TO DR. YORK

JANUARY 13, 1960.

Hon. HERBERT YORK, Jr.,
Director of Defense Research and Engineering,
Department of Defense, Washington, D.C.

DEAR DR. YORK: A few weeks ago you issued a statement to the press about Project Vela which dealt with the matter of improving the detection and identification of nuclear explosions. The purpose of this letter is to request specific information regarding this project.

Specifically, can you furnish me with answers to the following questions:

1. What research is being conducted at the present time and what is planned for the future regarding detection and identification of earthquakes and underground nuclear explosions?

2. What is being done to develop instruments to blot out background noise so that the direction of the first motion can be more clearly and more accurately read?

3. What research is being done to develop the possibility of placing seismographs in deep holes to blot out background noise?

4. What is being done to improve our knowledge about calculating the depth of earthquakes?

5. What is being done to improve our knowledge about the relationship of yield of nuclear explosion to earthquake magnitude?

6. What is being done to improve our knowledge regarding possible distinguishing characteristics of earthquakes and nuclear explosions to help identify the one from the other?

7. What is being done to make it possible to give quantitative values in terms of the identification of earthquakes to the diagnostic aids listed in the U.S. delegation's report of the recent technical conference at Geneva regarding the detection and identification of underground nuclear tests?

8. The Berkner Panel concluded that the analysis of long-period surface waves can probably identify about 50 percent of earthquakes equivalent to five kilotons or more. I notice, however, that in the report of the U.S. delegation on the technical conference it did not give any percentage estimate regarding the identification of earthquakes through the use of long-period surface wave analysis. Can you tell me whether the conclusion of the Berkner Panel still stands?

9. The Berkner Panel concluded that in considering the existing state of technology the Geneva net of 180 stations would acquire the same capability with improved equipment and techniques that can be specified today [in terms of numbers of unidentified events] for seismic events above 10 kilotons equivalent as was originally estimated by the Geneva conference of experts for seismic events above 5 kilotons equivalent. I gather that what this means is that with the two improvements recommended, i.e., increasing from 10 to 100 the number of arrays at each control station and through the use of the analysis of long-period surface waves, the Berkner Panel believes that 90 percent of the events of 10 kilotons or more would be identified. Is this the correct conclusion of the Berkner Panel?

10. The Berkner Panel concluded that a vigorous research panel in seismology would result in 3 years in such improvements that this would achieve the same capability in the Geneva net of 180 stations as was originally estimated by the Geneva conference of experts. Is this research program being pursued? Can we expect in 3 years' time the increased capability predicted by the Berkner Panel?

I would appreciate as detailed answers on these questions as you can give. I would also hope that they might be supplied within the shortest possible time because the subcommittee is planning to have hearings on these questions and this information is needed for them.

Sincerely,

HUBERT H. HUMPHREY.

INTERIM REPLY FROM GENERAL BETTS TO LETTER FROM SENATOR HUMPHREY

ADVANCED RESEARCH PROJECTS AGENCY,
Washington, D.C., January 25, 1960.

HON. HUBERT H. HUMPHREY,
U.S. Senate.

DEAR SENATOR HUMPHREY: Your letter of January 13, addressed to Dr. York, regarding Project Vela has been referred to me for the preparation of detailed answers to your questions. These answers will be forwarded to you by separate letter in the near future.

In the interim, I am happy to provide you general background information that may be of assistance. The Department of Defense has assigned management responsibility of Project Vela to the Advanced Research Projects Agency and funds in the amount of $8,535,000 have been allotted for research and development in the seismic detection field. The seismic program consists of the following major subdivisions:

(*a*) A research program designed to expand significantly the level of present seismologic technology in the United States, with emphasis on the generation and propagation of seismic waves.

(*b*) Exploration of new methods of obtaining precise seismic measurement, including new types of instruments, large arrays, use of subterranean instrumentation, and others.

(*c*) Selection and procurement of standardized seismographs and associated equipment for worldwide installation.

(*d*) Experimental installation of an inspection station with the specifications already agreed to at Geneva, together with one containing certain improvements recently proposed, in order that a comparative technical analysis may be completed.

(*e*) A program of underground nonnuclear chemical explosions to provide additional research data on seismic waves.

The Department of Defense is engaged in a continuous analysis of the problem of detection of nuclear detonations. The results of this analysis are provided to other executive departments and agencies to assist in policy review and formulation.

Sincerely yours,

A. W. BETTS,
Brigadier General, USA, Director.

SUBSEQUENT REPLY FROM GENERAL BETTS

ADVANCED RESEARCH PROJECTS AGENCY,
Washington, D.C., February 11, 1960.

HON. HUBERT H. HUMPHREY,
U.S. Senate.

DEAR SENATOR HUMPHREY: This Agency has received a telephonic request from your staff to advise you of the classification of individual parts of our letter to you dated January 25, 1960. The answer we provided to question 1 is confidential; the balance of the letter is unclassified. For your use if desired, a suitable unclassified answer to question No. 1 is as follows:

1. *What research is being conducted at the present time and what is planned for the future regarding detection and identification of earthquakes and underground nuclear explosions?*

Research and development programs in the broad areas of seismology have been supported by the Department of Defense at the level of about $1 million annually over the past few years. A substantial portion of this money has been

used to support programs which apply directly to detecting and identifying earthquakes and explosions. In addition, the Department of Defense is instituting a significantly augmented program in seismology, based on the recommendations of the Berkner Panel. The major areas being supported are as follows:

(*a*) Procurement and installation of standard calibrated seismographs at a number of selected stations.

(*b*) Basic research programs on the generation and propagation of seismic waves.

(*c*) Research on improved detection methods, such as improved seismographs and operation of seismographs in deep holes.

(*d*) Installation and operation of two seismic stations, one of the type recommended at the Geneva Conference of Experts, and one employing the improvements recommended by the Berkner Panel.

(*e*) A program of chemical explosions to support seismic research.

(*f*) Short-term special studies.

2. *What is being done to develop instruments to blot out background noise so that the direction of the first motion can be more clearly and more accurately read?*

Seismographs have been developed, and were used during Operation Hardtack and other U.S. test operations, which give excellent results under average noise conditions. As part of the work in support of the Berkner Panel's deliberations, arrays of up to 20 seismometers were tested by the Department of Defense at one location with the result that the signal-to-noise ratio was further improved by about three or four times over that obtained from a single seismometer. As part of the Vela program a contract is being negotiated to construct a station to be equipped with the full complement of seismographs as recommended by the Geneva Conference of Experts—the performance of this station can be evaluated beginning about June 1960. Additional work on the development of large arrays will be initiated soon as part of the Vela program. Fundamentally, however, it should be recognized that progress in this area requires new knowledge of the spectral composition of microseismic noise and first motion signals, and of the vibratory mode of short-period noise. Following this work, new instruments and new systems of instruments can be specified to give optimum discrimination between noise and signals.

3. *What research is being done to develop the possibility of placing seismographs in deep holes to blot out background noise?*

A research program has been underway since 1945 to investigate and develop this technique. However, major engineering problems remain to be solved. These problems are in two categories: maintenance of the deep hole; and improving the reliability of the instrumental probe. A high proportion of the operating costs have been required to keep the deep well open and to reopen it after frequent cave-ins or to remove accumulated solids which build up on the walls and prevent access to points below in the well by reducing the diameter of the hole. The instrumental problem is a difficult one, since the seismometer, which is of far greater sensitivity and delicacy than any which operate on the surface of the earth, must be calibrated and kept in operation even though it is thousands of feet removed from the operator, in a hot and corrosive chemical environment. The seismological data obtained to date are consistent with the theory, which predicts that the noise should decrease rapidly with depth. It is planned to continue this work under the Vela program.

4. *What is being done to improve our knowledge about calculating the depth of earthquakes?*

Studies of the depths at which shallow earthquakes occur, and of methods for determining these focal depths, will be supported under the Vela program.

5. *What is being done to improve our knowledge about the relationship of yield of nuclear explosions to earthquake magnitude?*

The existing data on magnitudes of nuclear explosions have been reevaluated continuously in light of theoretical calculations on the generation of elastic waves by explosions. It seems doubtful that significant improvements in our understanding can be reached without further underground nuclear explosions, as recommended by the Berkner Panel.

6. *What is being done to improve our knowledge regarding possible distinguishing characteristics of earthquakes and nuclear explosions to help identify the one from the other?*

Studies of the existing data on earthquakes and explosions are continuing within the Department of Defense. New work at an intensive level is being

initiated under Project Vela. We expect that significant advances in this area will result after several years of fundamental research. In addition, a program of nuclear and large chemical explosions, as recommended by the Berkner Panel would aid significantly in our understanding of the identification problem.

7. *What is being done to make it possible to give quantitative values in terms of the identification of earthquakes to the diagnostic aids listed in the U.S. delegation's report of the recent technical conference at Geneva regarding the detection and identification of underground nuclear tests?*

University and research laboratories with competence in the fields of seismology and data analysis, and with personnel who have participated as advisers in the negotiations on nuclear test suspension, are being contacted at the present time to establish projects for conducting such evaluations. It is anticipated that preliminary results will be obtained within the next 3 months. The final evaluation, however, can only come as a result of the more careful approach of Project Vela, subsequent to the acquisition of new basic data.

8. *The Berkner Panel concluded that the analysis of long-period surface waves can probably identify about 50 percent of earthquakes equivalent to 5 kilotons or more. I notice, however, that in the report of the U.S. delegation on the technical conference it did not give any percentage estimate regarding the identification of earthquakes through the use of long-period surface wave analysis. Can you tell me whether the conclusion of the Berkner Panel still stands?*

The comments of the Berkner Panel on the value of the analysis of long-period surface waves started in part: "This provides strong support for the belief that this technique can probably identify earthquakes of magnitude comparable to Logan and Blanca in about 50 percent of cases." Presumably, the members of the Berkner Panel still support this judgment. However, a quantitative estimate of the potential value of such analysis cannot be made without the research which the Panel stated as necessary.

9. *The Berkner Panel concluded that in considering the existing state of technology the Geneva net of 180 stations would acquire the same capability with improved equipment and techniques that can be specified today (in terms of numbers of unidentified events) for seismic events above 10 kilotons equivalent as was originally estimated by the Geneva Conference of Experts for seismic events above 5 kilotons equivalent. I gather that what this means is that with the two improvements recommended, i.e., increasing from 10 to 100 the number of arrays at each control station and through the use of the analysis of long-period surface waves, the Berkner Panel believes that 90 percent of the events of 10 kilotons or more would be identified. Is this the correct conclusion of the Berkner Panel?*

Our understanding of the Berkner Panel conclusions and the estimates of the Geneva Conference of Experts indicates that your conclusion is approximately correct.

10. *The Berkner Panel concluded that a vigorous research panel in seismology would result in 3 years in such improvements that this would achieve the same capability in the Geneva net of 180 stations as was originally estimated by the Geneva Conference of Experts. Is this research program being pursued? Can we expect in 3 years time the increased capability predicted by the Berkner Panel?*

The research program under Project Vela is based on the recommendations of the Berkner Panel. Work has been initiated on a few projects, and further intensive work will commence in the immediate future. While one cannot predict with certainty the results of a research program, we are undertaking the work with the same hope as that expressed by the Berkner Panel: "The PSI anticipates that in 3 years these efforts will probably result in further improvements which could achieve the same capability in the Geneva net of 180 stations as was originally estimated by the Geneva Conference of Experts (for shots coupled as in Rainier)."

Thank you very much for your interest in the Vela program; I hope the information supplied will prove of value to you and to the subcommittee.

Sincerely yours,

A. W. BETTS,
Brigadier General, USA, Director.

LETTER FROM SENATOR HUMPHREY TO ADMIRAL KARO

JANUARY 13, 1960.

Rear Adm. H. ARNOLD KARO,
Director, Coast and Geodetic Survey,
Department of Commerce, Washington, D.C.

DEAR ADMIRAL KARO: The purpose of this letter is to inquire what specific research has been conducted and is being planned for the future regarding improvement in techniques in the detection and identification of earthquakes and underground nuclear explosions.

Can your Division of Seismology furnish me with answers to the following questions:

1. What research is being conducted at the present time and what is planned for the future regarding detection and identification of earthquakes and underground nuclear explosions?

2. What is being done to develop instruments to blot out background noise so that the direction of the first motion can be more clearly and more accurately read?

3. What research is being done to develop the possibility of placing seismographs in deep holes to blot out background noise?

4. What is being done to improve our knowledge about calculating the depth of earthquakes ?

5. What is being done to improve our knowledge about the relationship or yield of nuclear explosion to earthquake magnitude?

6. What is being done to improve our knowledge regarding possible distinguishing characteristics of earthquakes and nuclear explosions to help identify the one from the other?

7. What is being done to make it possible to give quantitative values in terms of the identification of earthquakes to the diagnostic aids listed in the U.S. delegation's report of the recent technical conference at Geneva regarding the detection and identification of underground nuclear tests?

8. The Berkner Panel concluded that the analysis of long-period surface waves can probably identify about 50 percent of earthquakes equivalent to 5 kilotons or more. I notice, however, that in the report of the U.S. delegation on the technical conference it did not give any percentage estimate regarding the identification of earthquakes through the use of long-period surface wave analysis. Can you tell me whether the conclusion of the Berkner Panel still stands?

I would appreciate as detailed answers on these questions as you can give. I would also hope that they might be supplied within the shortest possible time because the subcommittee is planning to have hearings on these questions and this information is needed for them.

I have directed similar letters to other executive agencies so that my inquiry to you should be considered to request only that work being done either by or for the Coast and Geodetic Survey.

Sincerely,

HUBERT H. HUMPHREY.

REPLY FROM ADMIRAL KARO

U.S. DEPARTMENT OF COMMERCE,
COAST AND GEODETIC SURVEY,
Washington, D.C., January 20, 1960.

Hon. HUBERT H. HUMPHREY,
U.S. Senate, Washington, D.C.

DEAR SENATOR HUMPHREY: In reply to your letter of January 13, 1960, information relative to research being conducted by the Coast and Geodetic Survey in the field of seismology is given in the following paragraphs. The answers to your specific questions are numbered in the same manner as your initial inquiries.

1. This bureau is at the present time operating many seismographs, some of which are highly sensitive and capable of detecting, but not positively identifying, distant earthquakes and underground nuclear explosions as low as 10 kilotons.

Measurements of the ground effects from the Ranier and Hardtack II shots have been made. From the latter, a wave which follows the initial wave by one-fourth of a second but is in opposite phase, has been identified. This wave, which is believed to be a reflection from the surface near the point of detonation, could easily obscure identification of direction of first motion at distant stations.

We have measured ground effects from underground HE explosions at the PRE-GNOME site and are now participating in AEC Project Cowboy in Louisiana. The objectives of the latter program are to obtain sufficient data for studying the energy distribution from HE explosions in salt mines. Different weights of explosives in different types of holes are being used for the research work.

2. This bureau has conducted a statistical and mathematical analysis of array seismology, using wavelength filters to blot out background noise. According to our calculations, a well-placed and well-chosen array would reduce noise in the 3- to 20-cycles-per-second range, 100-fold or better. The investigation has advanced to the stage where we feel a field experiment should be conducted to supplement the theory. The attached proposal on "Array Seismometry" has been submitted.

3. In the past this bureau has operated instruments at the bottom and near the surface of a 5,000-foot mine shaft, recording earthquakes in both places. At the bottom of the shaft a 10-to-1 signal-to-noise advantage was found for 1-second waves or less, but there was no decrease for 4-second microseisms. Although we have not conducted specific research in deep-hole seismology in recent years, we have submitted the attached proposal for conducting this type of experiment.

4. The determination of the depth of earthquake foci would be a specific study under our research proposal to digitize earthquake data on an electronic computer. This bureau has completed the mathematical calculations necessary for determining epicenter locations by electronic computer procedures. We are now testing the method, using various earthquakes as samples, and expect in the near future to determine the location of all epicenters by this means. Further research on problems associated with this program, for example, the depth of foci, is contingent upon favorable action in a proposal to the Air Force, or on some other source of funds.

We also propose to develop a seismometer with a higher frequency response, 5 to 10 cycles per second, than the conventional seismometer of 1 to 2 cycles per second. Such an instrument would possibly give better definition to pP waves, which could be read to a fraction of a second on the seismogram.

5. When we have measured the seismic energy of HE and atomic explosions, we have computed the magnitudes by using established equations. Cross correlation has also been made by using the energy release of the explosions to compute magnitudes. Comparison of magnitudes from the two methods of computation makes it apparent that results are inconclusive because of unknown variable factors in the transmission of energy through the earth.

6. All seismograms for current and past operations have been thoroughly examined and studied with a view to finding distinguishing characteristics of earthquakes and nuclear explosions. Many of the research proposals and programs have the inherent objective of gaining knowledge on this point.

7. We have no information on this point.

8. We have no information on long-period surface waves.

For your information, copies of our proposals for research in the field of seismology are attached.

Sincerely,

H. Arnold Karo,
Rear Admiral, Coast and Geodetic Survey, Director.

Proposal No. 1

ARRAY SEISMOMETRY

1. Objectives

To reduce background noise (short-period microseismic or otherwise) through the use of an array of seismometers as a wavelength filter.

2. History

The Coast and Geodetic Survey has been operating seismographs since 1925, and modern seismographs (those in present use and in good standing) since 1940. A Coast and Geodetic Survey geophysicist has been working on the math-

ematical feasibility of arrays since 1948, and has given advice to certain organizations making limited use of arrays. He has finally arrived at a solution which is believed will allow a 40- to 60-decibel reduction in short-wave-length noise regardless of whether the noise is random or systematic surface waves. This may be accomplished by the use of Fourier transforms with appropriate weighting of the elements in the system. The Coast and Geodetic Survey has had no direct experience in operating arrays outside of tripartite nets, but proposes at first to engage exploration companies who have had such experience, and later to operate an array, if feasible, as a part of a seismological laboratory somewhere in the western mountains or plateau area. If it is not feasible to operate near the laboratory, the array will be set up in some other optimum area.

3. Discussion

An array of seismometers arranged in a square net, 3 kilometers and 14 units to a side, is essentially a wavelength filter and depends for its success on the fact that the speed of seismic waves having the same frequency is 2 to 2½ times or more greater than that of the noise crossing the array. It will also filter the noise which originates within the array. The array must be given precise size, spacing, and weighting functions for greatest reliability in distinguishing the direction of first motion when the signal is much weaker than the noise, and in measuring the PcP minus P interval for distinguishing surface explosions from normal depth earthquakes.

(*a*) *Preliminary work.*—Feasibility analyses have been done. It is proposed to use commercial systems in a preliminary survey. This will consist of deploying at least 172 geophones in a selected 3-kilometer square of granite outcrop in Texas in time to monitor proposed shots next spring. This will mean that seven commercial teams, each with twenty-five 4-cycles-per-second geophones, are to be used. Note that the above is six geophones over a 13 by 13 square system. The spacing of the geophones will depend on the site selected and the speed in the underlying rock. (See next section.) The records are to be straightforward, with subsequent sampling of the traces from which the feasibility of additional work is to be established.

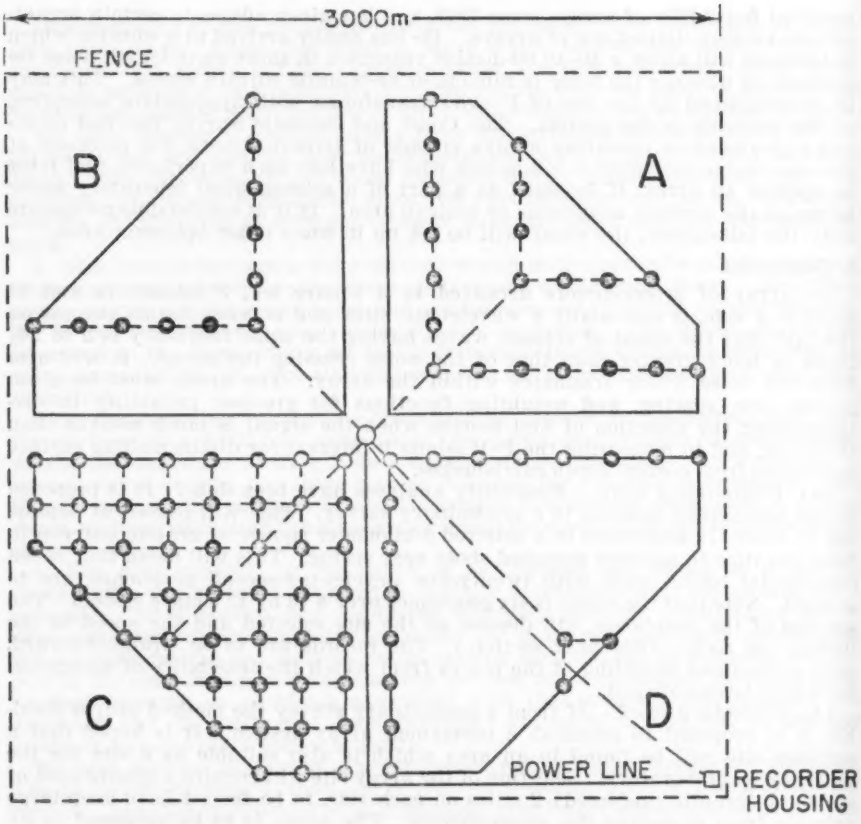
(*b*) *Secondary work.*—If from a preliminary survey the method proves feasible, it is proposed to establish a permanent array system. It is hoped that a suitable site will be found in an area which is also suitable as a site for the seismological laboratory. Selection of the array site will require a square area of scabland (granite preferred) 2 miles on each side, to be fenced in to keep large animals from molesting the seismometers. The array is to be arranged in an octagon as shown in the sketch, divided into four quadrants of 49 units each. Each unit is to be connected to a central distribution center by a twisted pair. Direct burial polyethyline No. 14 stranded copper is suggested. The quoted price is $86.50/k. ft. The proposed average spacing of the units is 200 meters as indicated in the drawing. From theory, the inner units will be spaced closer together and the outer elements farther apart. Wiring is according to the patterns shown in quadrants A, B, D, and a completed quadrant is as in C. Assuming equal spacing, the wire requirements for the net alone is 230 kilometers or 750 k. ft. with a 3-percent allowance for waste. A power line into the area and a telemeter line from the center of the area to the recording room will be extra. Actually the wiring requirements may be slightly less than this because of the concentration of the elements toward the center of the array.

4. Operation

It is believed that earth power to the switchboard at the center of the array will be sufficient. Since the units toward the center will be heavily weighted, the line resistance from each unit will be a step toward this end. Disturbances resulting from electrical storms is recognized. If preamplification becomes necessary, power must be supplied each unit, and a satisfactory means of amplification without noise must be provided. Moving coil seismometers in small buried vaults accessible from the top or contained in small canisters are proposed. One set of three-component short (and long) period seismometers with separate recording may be part of the equipment.

5. Location

Because of the large size and other exacting requirements, location of a satisfactory site will be difficult. A possibility, or a waste area elsewhere in the western desert, or possibly the western Dakotas or Montana.



PROPOSED SEISMOMETER ARRAY

PROPOSAL NO. 2

DEEP HOLE PROJECT

1. Objectives

To monitor seismic signals from several thousand feet underground in two areas of the West, one along a fairly active earthquake fault and the other at some distance from an active seismic area.

2. History

In 1950 and again in 1953, the Coast and Geodetic Survey measured or supervised the measurement of ground vibrations on the lower level of the Homestake mine in South Dakota at a depth of 5,000 feet. It was observed that although 4 second microseisms were recorded with about the same amplitude deep underground as at the surface, 1 second (1 c.p.s.) noise was attenuated at least 20 db at the deep station. P-waves from distant earthquakes were about 6 db lower in the mine than at the surface, leaving a signal to noise advantage of at least 14 db.

It is believed that background noise consists in large part of Rayleigh waves. This is probably true if the vertical component is relatively large. According to theory, and if the rock is homogeneous, attenuation of the vertical component is exponential, at first slow and then rapid. In units of depth of half wave lengths from $\frac{1}{2}\lambda$ to 3λ, the fall off with depth is respectively 5, 14, 25, 35, 46, and 56 db, or roughly 10 db for each half wave length beyond the first. It follows that noise drop is much more rapid in low speed sediments than in

crystalline rock, such as granite. The most favorable location therefore would be in an isolated desert valley, because it is recognized that although noise attenuation is much less in low-speed rock, there is more likelihood of noise if there are nearby sources of disturbances.

3. Personnel and organization

Seismologists of the Coast and Geodetic Survey have had experience and interest in deep well seismometry for more than 20 years. In 1938, an attempt was made to record strain noises from the bottom of 500-foot-deep wells near Hayward fault; one on the Mills College campus in Oakland, Calif., and the other at the zoo, a few miles southeast of Oakland. Instrumentation at that time was crude and insufficient to yield satisfactory results. The experience in a deep mine has been mentioned. In recent years much thought has been given to a combined project of listening for strain noises which may possibly lead to earthquake prediction, and of recording weak seismic signals from nuclear explosions and small distant earthquakes. Improved instrumentation and a larger background of experience will better insure the success of both of these objectives. The Coast and Geodetic Survey is adding to its staff a geophysicist who has had wide experience in exploration work in this country and abroad, including operation of seismometers in deep wells and making velocity logs, and who for several years has conducted seismological operations in the field under adverse conditions. With the combination of wide experience in conventional and exploration seismology, including numerous field projects, and an incentive to do the proposed work, the Coast and Geodetic Survey is in an excellent position to carry on a project of this type.

4. Proposal

(1) Two or three test sites are proposed:

(*a*) Near the San Andreas fault near Hollister, Calif.: It is understood that two or three wildcat wells have been drilled in that area. Whether or not they are available or usable is not known. This experiment may also test if strain noise precedes earthquakes. The area is suitable for the experiment because it is an active seismic zone.

(*b*) Yucca Flat, near Las Vegas, Nev.: The AEC is proposing to drill holes both in granite and other formations, 4,000 feet or more deep. One of these may be a suitable test hole when not in use.

(*c*) Near the proposed Coast and Geodetic Survey seismological laboratory.

(2) Instrumentation:

(*a*) *Seismometers.*—The following to be sealed in 1 package: (1) 1 each 1 c.p.s. moving coil (dynamic) seismometer; (2) 1 seismometer sensitive to frequencies 5–50 c.p.s.; (3) geophone or seismometer sensitive to frequencies 50–500 c.p.s. (2 or 3 may be in 1 package); (4) other sensing equipment, including a Hast stress gage, if considered feasible; (5) preamplifiers using direct current supplied from the surface; (6) Servo-motor or motors to adjust positioning of seismometer masses. It is anticipated that recovery of seismometers may not be practicable. Cementing them to the hole for good coupling will probably be necessary.

(*b*) *Recorders.*—To be on surface at top of well: (1) One or two magnetic tape recorders with provisions for 24-hour operation on each tape, recorder to be attached to the high frequency seismometers; (2) a standard film recorder to be attached to the 1- c.p.s. seismometer in the well and an identical one at the surface; (3) suitable playback, timing, and reading apparatus.

(3) Operation: By contract, or 2 full-time persons at each station. Duty status to be 7 days a week. If workload proves too heavy for 2 persons, additional personnel to be provided.

(4) Housing and quarters: While the program is in an experimental stage, minimum shelter at the seismograph site to be provided. However, nearby office space may be necessary.

(5) Preliminary and associated work: Thorough test checking of instrumentation to go in the hole will be carried out before it is placed. Other provisions will be made to overcome troubles associated with conditions several thousand feet deep. These troubles include high temperatures, high pressure, corrosion by chemical action, tube noise, system noise, and changes in gravity.

PROPOSAL NO. 3

STUDY OF EARTHQUAKE AFTERSHOCKS IN COMPARISON TO EXPLOSIONS

1. Introduction and statement of capabilities

The Seismology Branch of the Coast and Geodetic Survey has been engaged for a number of years in monitoring explosions in this country and in the islands of the Pacific. This monitoring has included detection, travel time, wave propagation, and energy determinations of explosions and their relationship to earthquakes. The Survey has frequently established temporary seismograph stations (strong-motion) to investigate aftershocks of strong earthquakes along the west coast and has conducted extensive field investigations of the faulted areas. In view of the experience gained in this work, the Survey wishes to conduct intensive studies of the many problems relating to explosions and earthquakes.

The Survey has a number of geophysicists who are experienced in establishing temporary and permanent seismograph stations including site selections, station construction, instrument operation, analysis of seismograms, and related research. They are also experienced in the operation of the various types of equipment with period ranges from 0.01 second to 50 or 100 seconds. They have had considerable experience in establishing strong-motion stations for monitoring California, Montana, and other west coast earthquakes. This may be readily adapted to one required for monitoring earthquakes to determine wave characteristics from normal and deep focus earthquakes, energy release and attenuation in various geological formations. Similar work employing strong-motion and teleseismic equipment has been done with explosions in the Western United States and the islands of the Pacific

The Survey has several permanent observatories near active earthquake areas in Montana, Nevada, Utah, and Alaska. These would form the nucleus about which other permanent and temporary stations could be established. In addition there is the strong probability the Survey will have a six-station telemetry system with either onsite recording or some form of telemetering recording. This system, being portable, would be available for moving into the earthquake area soon after the main shock has occurred for recording all significant aftershocks. Crystal clocks would be employed for more precise timing and for control of recording drums, thereby providing much greater accuracy for time measurements and velocity studies.

2. Objectives

The objectives of this proposal are to provide adequate seismological equipment and other related equipment to study the characteristics of natural earthquakes and to investigate specifically such problems as aftershock patterns, focal mechanics, energy distribution and attenuation, wave characteristics, and amplitude relations of P and S for dependence on azimuth from source, focal depth, and magnitude of source energy; to conduct triangulation measurements, including level and horizontal displacements; to measure tilt close to the defined faulted area.

3. Method of approach

The Survey will collaborate with existing stations near the epicentral area whether in California, Montana, Nevada, etc., to obtain the best preliminary location of the main shock. This information together with the evidence from immediate field investigations will determine the location of the temporary stations to be installed within 24 to 48 hours. The instrumentation to be used is the six-component six-station system proposed by LRL for monitoring explosions. This portable system when not engaged in explosion work would be employed in recording earthquake aftershocks. The six-station system is designed for on-the-spot recording, employing photographic paper. The stations will be established in at least three directions from the epicenter, using as backup stations any existing permanent seismograph stations and the temporary long-period recording stations. The time accuracy for the recording of significant P phases will be at least 0.1 second and the epicentral position should be determined within the limits of error required for research investigations, possibly within a kilometer. In addition, three-component long-period seismographs will be temporarily installed in three permanent-type structures (existing buildings) within 200 miles of the epicenter.

As soon as it is determined that there is little or no more danger from subsidence, landslides, flooding, etc., a triangulation party will enter the epicentral area to obtain earth movements by first-order triangulation (if feasible) for horizontal displacements and by leveling for vertical displacements. This work is contingent upon the geographic location of the epicenter (i.e., feasibility of such field work) and previously available geodetic measurements. If no such measurements are available in the area, a decision must be made whether it is practical to undertake them.

The use of tiltmeters are to be investigated further for earth measurements near the fault system. It is planned to investigate the feasibility of commercially available tiltmeters and to operate any found promising in this program. This equipment will be installed along with the seismographs immediately after a major shock has occurred.

4. Instrumentation

The instrumentation will consist essentially of the six-component six-station system which will be portable and will operate on a battery system on a trickle charger from a gasoline generator. The seismometers will be HTL geophones and small model Benioffs with frequency coverage between 1 to 100 cycles per second. The three stations with long-period recording seismographs will use the Columbia type seismometers and the Lehner and Griffith long-period galvanometers. The recording will be on photographic paper.

For the triangulation work the established equipment of the Survey party will be used for the first-order triangulation and precise leveling measurements.

Proposal No. 4

Equipping of selected stations with modern seismic equipment

1. Introduction and statement of capabilities

The Seismology Branch of the Coast and Geodetic Survey has particular interest in this research project as the establishing and maintaining of seismograph stations in many areas of the world has been one of its functions. The Survey maintains a system of 25 stations from the South Pole to Alaska and has direct contacts with the operations of several hundred other stations in the world. Close contact is maintained with most stations of the world through the earthquake-locating program that has operated for 25 years. Through international exchange programs and natural grants South American and Asia countries have sent seismologists to the Survey for training and experience in station seismology. Because of the international aspects of the Survey's work and the extensive experience of its scientists it is in a favorable position to do this work.

The Survey has a number of geophysicists who have experience with nearly all types of seismographic equipment including mechanical, electromagnetic, radio telemetry systems, and electronic systems. Moreover, several have had experience in developing much of the equipment the Survey uses in its network of systems. With this specialized experience in knowing the construction of the equipment, having installed it in observatories in the United States and other parts of the world, having maintained and serviced seismographs for years, and having trained other observers, the Survey's geophysicists are qualified in this field. In addition to the technical experience the Survey has worked very closely with the State Department and the military departments in making personal contacts with foreign seismologists, in exchanging seismic data and in establishing seismic stations in foreign countries. It is also experienced in handling shipments of seismographs through General Services Administration; through Customs (particularly in South America) and has the advantage of good relations with American Embassies and consulates.

The Survey is limited in the size of its physical laboratory at the present time. Space can readily be obtained for training additional geophysicists, checking, and preparing equipment for shipment and other necessary operations preparatory to installations in this country and in foreign areas. There would be no particular problem in finding space in the Washington area if it is required to meet the instrument requirements that are in excess of the Survey's capacity.

2. Objectives

The objectives of this proposal are the program planning, the procurement of first-class instrumentation; installation in approved seismograph stations; the training of personnel in operation and maintenance of the instruments, and

the periodic inspection and service of the instruments during succeeding years. These objectives are similar to those of the established program of the Survey wherever it establishes new stations.

The establishment of first-class instrumentation in stations having experienced personnel and suitable buildings will provide a definite impetus to research in all fields of seismology because of (1) increased capabilities of first-class instrumentation; (2) instrument standardization and known calibration; (3) greater accuracy of time control; (4) uniform rate of drum speeds; (5) simplification of seismogram reading because of similarity of recording.

3. Program procedure

The Survey with its detailed knowledge of the active seismograph stations is in a unique position to select those to participate in this program. The Survey having direct contact with leading seismologists and organizations can readily supplement its fund of information with firsthand recommendations from them. In procuring the recommended instrumentation, the Survey can negotiate or contract with the leading U.S. manufacturers with whom there have been close relations. The Survey has also had extensive experience in packing, crating, and handling cross country and oversea shipment. This is particularly important for the oversea shipment, as replacement parts will not be readily available.

4. Instrumentation

It is understood the standard instrumentation will be a three-component short-period seismograph and a three-component long-period seismograph. The recommended instrumentations are the short-period variable reluctance Benioff seismometer with multiple pickup from the coil or coils and the Columbia or Press-Ewing long-period seismometers or their equals.

The galvanometers will be the Lehner and Griffith short-period and long-period systems or equals. The recording will be on photographic paper and the time control and drive for the recorder will be by crystal clocks. The crystal control is recommended because of the usage of various power frequencies in foreign countries. Adequate radios depending on station site and source of time signals and miscellaneous items will also be supplied to each station.

The Survey is also including in the proposal an item for improving the efficiency of two stations by use of telemetry stations where the pickups will be a few miles from the recording vaults; for the construction of a vault in Mount Rushmore National Park, and for the salary of a part-time employee. The instrumentation and installation services at Mount Rushmore will be provided by the Survey.

PROPOSAL No. 5

THE ADAPTATION OF COMPUTER METHODS TO SEISMIC INVESTIGATIONS

1. Introduction

This proposal is concerned with the adaptation of an available method of locating epicenters by digital computer to improving the accuracy of seismic travel time curves and to the processing of data "read in" to the computer directly from selected seismometers. In its present form, this method is perfectly general in that the locations are made from only P arrival times with no other intelligence necessary. It tests the accuracy of each reading and the overall statistical accuracy of the epicenter. Origin time to an accuracy of ± 0.1 second, latitude and longitude to the nearest 0.1° and depth to the nearest 10 kilometers are given. Additionally, distance, azimuth, and statistical variation (residual) are given for each station reporting an epicenter.

For the past 15 years, the Coast and Geodetic Survey has maintained a rapid location program. It has established a worldwide communication network which has been constantly improved through the years by the gradual addition of strategic stations, the establishment of more rapid data transmission schedules, and a general improvement in the quality of the data. The resulting epicenters were promptly distributed to cooperating institutions and other interested organizations on a current basis. This service and the contacts thus made have been important factors in maintaining interest and intercommunication among seismologists of the world. Under this program, 75,000 readings are processed and about 1,400 epicenters are located each year.

Seismologists in the Seismology Section have acquired a thorough knowledge of the seismicity of the world with its anomalous conditions, the relative efficiency of the reporting stations, the computational methods available to the

problem and their relative merit and the limitations imposed by the data and the methods used. They have also gained wide experience in soliciting and maintaining foreign cooperation and communications.

The computer section is composed of geodesists, mathematicians, and statisticians having a wide experience with digital computers and auxiliary machines. Many of them have been trained by IBM and a close contact is maintained with that company for consultant services.

Programs developed by this section include those for the calculation of plane coordinates for geodetic work, space coordinates, geodetic triangulation involving calculations of distance, and azimuth and simultaneous solutions using the Cholesky method and photogrammetric navigation and positioning problems.

2. Objectives

The following objectives will be taken up in the order stated below:

(*a*) To catalog residuals for each epicenter for each station used and when enough data is accumulated to establish station weights.

(*b*) To refine the travel time curves using statistical averages derived from cataloged residuals.

(*c*) To study anomalous variations in travel times for specific regions of interest.

(*d*) To process epicenters to a much greater degree of accuracy than now possible in regions of special interest. This accuracy will be to plus or minus 5 kilometers using worldwide travel times and should be considerably better using adjusted regional curves.

(*e*) To develop discriminating techniques to enable the computer to assign each reading to a specific earthquake without a human agency thus allowing automatic "read in" from a selected group of monitoring stations. The capacity of the IBM 650 using continuous "read in" is about 500 locations per day. This rate could be increased considerably using a larger computer.

(*f*) To catalog phase amplitude ratios from standardized instruments using the better distances available from the above processes to study characteristic attenuation patterns. This would be done in order to establish differences between earthquake and explosion records.

Proposal No. 6

Study of Microseismic Noise

1. Introduction

The seismological program of the Coast and Geodetic Survey is concerned with the study of all phenomena related to earthquakes and the propagation of seismic waves. In this, the problem of microseismic background becomes an important factor because microseisms frequently mask pertinent earthquake phases and at times completely dominate the records. A number of papers on microseisms have been written by geophysicists of the Bureau, which includes a recent terminal report of the U.S. Navy microseismic program evaluating the usefulness of microseismic data for tracking and detecting storms at sea.

However, many basic facts concerning microseismic origin and propagation are as yet unexplained. It is proposed therefore to undertake a research program with the following objectives:

(*a*) Development of new or refinement of existing techniques for recording microseisms through array systems. This should permit analyses of individual waves and azimuth determinations of source areas. Systems should contain instruments of varied response characteristics for recording complete microseismic spectra. A 9-unit array with location to be decided later, with magnetic tape recording is recommended.

(*b*) Development of a deep water pressure recorder capable of detecting small fluctuations in pressure on the ocean bottom which are postulated by the wave interference theory. If such pressure fluctuations are recorded, an adjacent ocean bottom seismometer should reveal whether or not these pressures create seismic surface waves. It is proposed to establish a temporary station on land adjacent to deep water and to record simultaneously pressure fluctuations in deep and shallow water, and microseisms from the ocean bottom and at the station. Cables leading to the underwater equipment will be necessary. Later, recording in deep water far from land may be attempted.

(*c*) An extensive study of microseismic attenuation across certain geological structures or areas such as oceanic basins, continental margins, mountains, and geological faults. If the study reveals the existence of microseismic bar-

riers, it should be possible to find favorable sites for seismograph stations which could operate with greatly reduced noise levels. Several geophysicists experienced in microseismic research are available to direct this research program.

Development or modification of seismic equipment would be carried out by technicians in the Seismology Laboratory. Their experience in developing, testing and installing seismological instruments for teleseismic and strong-motion programs well qualifies them for this phase of the research program.

All oceanographic instruments would be developed by oceanographers and technicians in the Tides and Currents and Instruments Divisions of the Coast and Geodetic Survey. These divisions have had wide experience in the development and operation of tide gages, seismic sea wave detectors, and related oceanographic equipment.

The facilities available for conducting this program include the following:

1. The Coast and Geodetic Survey Seismological Laboratory.

2. The fleet of Coast and Geodetic Survey ships which would be utilized for the installation and operation of all oceanographic instruments. The major operational activities of these ships are the hydrographic surveys along the Pacific, Atlantic, and gulf coasts, Alaska and the Hawaiian Islands.

3. The present net of Coast and Geodetic Survey and cooperating seismograph stations where existing facilities could be used for limited microseismic studies. This network includes the Antarctic stations of Byrd and South Pole, Guam in the western Pacific, College in central Alaska, and San Juan in the West Indies. Inasmuch as these and other stations maintain adequate time control and experienced observers, expansion of existing facilities could be accomplished at minimum expense with anticipation of accurate results.

Proposal No. 7

Study of Macroseismic and Microseismic Noise Levels

1. Introduction and capabilities

The Coast and Geodetic Survey is interested in conducting background noise studies because it is a prerequisite for its seismic operations. Data have been compiled during the years from the results of operating conventional instruments under the most favorable conditions. Data for the amplitudes and periods of average storm microseisms, of traffic and industrial vibrations, of artificial and nuclear explosions are available for limited energy sources, periods, geological conditions, and foundation factors. These results do not suffice. More data under almost any conceivable operating condition are needed for the seismic programs envisioned during the next few years.

The Survey has compiled considerable statistics of noise levels by scaling microseismic storms at conventional seismograph stations, by scaling seismograms resulting from site tests across the country, Alaska, Hawaii, and islands of the Pacific, by monitoring explosions in several parts of the world. In spite of all the accumulated seismograms there has been little analysis of these noise levels. Before new and additional observations are accumulated those already available should be investigated. The Survey has a good source of these seismograms from stations halfway around the world. In 1947, the Survey reactivated a system of reporting microseismic readings from stations around the world. The response was encouraging as it was intended for 1 or 2 years. A similar program channeled for specific observations over short periods of time would be productive for this noise study.

The Survey has had experience in conducting these studies and has some instrumentation available. As the instrumentation must be calibrated, the Survey is in a favorable position as it has determined response curves for all of its instruments. The Survey has also collaborated with laboratories on the west coast, in Texas, and in Washington for extending its efforts in this calibration work. In view of this positive need and because of its capability the Survey will have its equipment standardized.

2. Objectives

It is proposed to study noise levels in this country and in some oceanic island areas with conventional and specialized instruments that are thoroughly calibrated and standardized. Seismic amplitude spectrums will cover a frequency range of 100 c.p.s. to 0.05 c.p.s. with frequency resolution, and will include amplitudes as low as millimicrons. The field measurements will be taken over sufficient time lengths so as to give minimum, average, and maximum noise conditions for horizontal and vertical ground movements.

Systematic studies will be conducted for atmospheric disturbances causing ground disturbances; industrial noises, explosions, and other sources. Included in the conditions under which the observations will be made are differences in geological structure, considering great units of rock formations like the Canadian Shield as contrasted with fractured systems; differences in surficial geology (foundation factors, climatological and atmospheric factors, and geographic sites (near shore, midcontinental, oceanic), near high density culture and in remote areas). It is anticipated that a sufficient number of stations will be occupied so that a contour method of revealing the various parameters or quality of signal to noise ratio may become meaningful.

3. Method of approach and instrumentation

The Survey proposes to review the literature reported in this field and compile these available data along with those data including storm microseisms and other noises recorded on seismograms obtainable from representative stations of the world. It also proposes to use its own direct recording tape equipment with geophone type detectors and commercially available equipment such as a VLF amplifyer bank and recording on Ampex magnetic tape and on a Brush-type recorder. For the low-frequency waves the three-component Columbia-type seismometers and the conventional photopaper recorders will be used.

The field investigations will be planned in advance as to the objectives and best means for accomplishing them. It is hoped that all observations may be conducted under existing conditions for protecting the instrumentation. There is no plan for temporary housing. The seismic noise team will establish its seismic equipment at a particular site, make its own recordings for short periods of time or if daily observations are to be made, obtain the assistance of small local technicians, students, etc., to operate the instruments periodically for limited periods of time. It is possible the team can set up the equipment at various sites or under different operating conditions, for instance, on granite, conglomerate, alluvium, etc., at various distances from the energy sources such as surf, machinery vibrations, stonecrusher operations, and heavy truck traffic. It is planned that some magnetic tape data will be available for special frequency analyses. From the surveys conducted it is hoped that sufficient data will be available to give a good perspective of the requirements for operating seismographs of moderate to high gains in the frequency range of 100 to 0.05 c.p.s. under nearly every conceivable operating condition.

MEMORANDUM TO DISARMAMENT SUBCOMMITTEE FROM THE ATOMIC ENERGY COMMISSION RE PROJECT COWBOY ON THE CONCEALMENT OF UNDERGROUND TESTS

U.S. Atomic Energy Commission,
Washington, D.C., January 8, 1960.

Miss Betty L. Goetz,
Staff Assistant, Disarmament Subcommittee,
Committee on Foreign Relations, U.S. Senate.

Dear Miss Goetz: Recently you asked Mr. Robert O'Neill of the Atomic Energy Commission's Congressional Liaison Office for information on the high-explosive experiments being conducted in a salt mine in Winnfield, La. I shall give in the following paragraphs a description of the work accomplished to date, the results therefrom and our plans for the future.

The high-explosive charges fired to date are the first of a series of 10 or more experiments. They have consisted of the firing of two 20-pound high-explosive charges on December 17, and of two 100-pound charges on December 19. In each case, one of the two charges was fired as a tamped explosion, i.e., no significant airspace or void surrounding the high explosive. The other firings in each case were in a spherical chamber several feet in diameter. Seismic instruments were placed in the vicinity of the firings and out to a distance of about 8 miles to investigate the nature of the seismic signals created, and to determine whether placement in the large cavity would tend to decouple, or damp out, the seismic signals of interest.

Initial analyses of the data from the firings of the 20-pound shots indicate that decoupling occurred and that the factor of decoupling for these small chemical explosions in salt was greater than 40 for the significant seismic frequencies recorded. The initial analyses of the 100-pound firings confirmed that

decoupling did occur at the same or greater factor. The data from the various instruments are undergoing more thorough analysis at present by the Lawrence Radiation Laboratory and the Rand Corp. to better refine the quantitative data and their significance, particularly as applicable to nuclear detonations. A report of the more detailed analyses will be available within the next few weeks and I shall see that you are informed of the results.

We intend to proceed with further similar experiments using charges of increasing size. We expect that the next experiment will be conducted on or after the 20th of January, and we shall progressively increase the size of charges until firings of the order of 5 tons of high explosive are made.

It is my understanding that you asked Mr. O'Neill also for a copy of the summary statement prepared by members of the U.S. technical delegation at the Geneva talks and made at the West Coast Annual Meeting of the American Physical Society on December 29, 1959. I am attaching two copies of that statement.

I hope that the above information will meet your needs. Should there be additional information that you require, please call upon me.

Sincerely yours,

A. R. LUEDECKE, *General Manager.*

MEMORANDUM TO DISARMAMENT SUBCOMMITTEE FROM THE ATOMIC ENERGY COMMISSION RE PROJECT COWBOY ON THE CONCEALMENT OF UNDERGROUND TESTS

U.S. ATOMIC ENERGY COMMISSION,
Washington, D.C., February 1, 1960.

Miss BETTY L. GOETZ,
Staff Assistant, Disarmament Subcommittee,
Committee on Foreign Relations, U.S. Senate.

DEAR MISS GOETZ: On January 8, 1960, I wrote to you about the initial analysis of the results of the Cowboy 20-pound high explosive detonations. Enclosed is a more detailed report prepared recently by the Livermore Radiation Laboratory and the Rand Corp. on the preliminary analysis of the subsequent Cowboy 100-pound high explosive detonations which occured on December 19, 1959.

Other high explosive detonations have occurred in the Cowboy series since that time. On January 23, 1960, 200 pounds of high explosive were detonated in a large hole, for instrument calibration purposes. This past Saturday morning, January 30, two 200-pound charges were also detonated. One was in the hole and the other was fully tamped. We do not as yet have any technical results on these firings.

We expect that additional and more thorough analyses of the past and subsequent Cowboy events will be available after the completion of the Cowboy series.

Sincerely yours,

A. R. LUEDECKE, *General Manager.*

A PRELIMINARY ANALYSIS OF COWBOY, PHASE I

(Prepared by Livermore Radiation Laboratory and Rand Corp.)

INTRODUCTION

A purpose of the Cowboy experiments is to check some aspects of the prediction that a nuclear explosion in a large, deep cavity will give a seismic signal much smaller than the same explosion carried out in a small room. There are two points which should be understood before examining the Cowboy results.

First one should understand exactly what the prediction about decoupling is. It is as follows: The distant seismic signal from a nuclear explosion in a properly constructed large cavity in salt or hard rock will be about 350 times smaller than the signal generated by a nuclear explosion of the same yield detonated under the same conditions as the Rainier explosion. The Rainier explosion took place in Nevada in a small room in the volcanic rock called tuff. Or, more briefly, the decoupling factor due to a hole in salt compared to a fully tamped—i.e., in a small room—explosion in tuff is about 350. This prediction is a limited one.

It deals only with distant signals which contain only low frequency seismic waves less than about 3 cycles per second, and the prediction does not necessarily apply to higher frequencies. It does not give the decoupling factor for a hole in salt compared to a tamped explosion in salt rather than tuff. Further, it does not give the effect of changing from nuclear explosives to chemical explosives. Since the effect of moving the tamped shot from tuff to salt and of changing from nuclear explosives to chemical ones is not known, the decoupling factors for Cowboy cannot be exactly predicted; however, these effects are expected to be modest so that one still expects a large decoupling factor. It is only the decoupling factor with respect to tuff which is important since it is tuff explosions which have given the basic data for the Geneva system.

Secondly, one should realize the possibilities of small experiments are limited. The signals generated will be quite small. They will not be seen at large distances where we are principally interested. Even at lesser distances they will not be much larger than the noise; this is particularly true for the decoupled shots.

In other words, we cannot expect all questions to be settled by these experiments. However, considering the data which we have analyzed, the experiments have increased our confidence in the prediction of a decoupling factor of about 350 for salt hole explosions compared to tamped tuff explosions.

COWBOY PHASE I

Phase I has consisted of four detonations, two shots of 20 pounds of high explosive and two shots of 100 pounds of high explosive. Each set had a fully tamped and a decoupled shot. We will discuss hereafter the 100-pound shots since they give the largest and most easily interpreted signals.

NEAR-IN MEASUREMENTS

In considering nuclear shots and Rainier data the calculation of the decoupling factor for the large hole is made in the following way: There are experimental measurements of the motion of the earth near to the Rainier detonation and also at great distances from it. One can also calculate the motion near a large elastic hole due to a nuclear explosion. Now we cannot at this time calculate exactly the distant signal from the near-in signal but if one knows near-in motion from two explosions and the distant signal from one of these explosions, the distant signal of the other can be estimated. Thus from the near and distant Rainier measurements and from the calculated values of the earth motion for a hole, the distant signal from a hole has been estimated. This calculation is simple and we are quite confident of it. The high explosive experiments will indicate something of the validity of these calculations.

In Cowboy there are instruments placed out to about 35 feet from the explosions which measure the motion of the earth at those points. The instruments near the tamped shots enable us to measure the difference between a high explosive shot in salt and a nuclear shot in tuff. The instruments near the hole enable us to check to see if our understanding of the behavior of the hole is correct.

These measurements are much more difficult than the similar ones performed near Rainier. This is due to the much smaller size of the explosions and to the higher frequencies generated by them.

MEASUREMENTS NEAR TO THE HOLE

The analysis of these measurements at high frequencies, around 300 cycles per second, appear reliable and are about what would be expected from our understanding of high explosive shot in a hole. At low frequencies, less than about 20 cycles per second, the data are confused by instrumental difficulties. The best present analysis of this data (less than 20 cycles per second) indicates a signal which agrees with theory. There is as yet no reliable data in the 20- to 100-cycle-per-second range.

While these measurements (less than 20 cycles per second) are uncertain, it is clear that the signal from the hole is small and there is nothing which would lead us to believe it is not what we have expected it to be. For this reason we will assume our theoretical calculations for the motion of the earth around the hole are correct and we will use these motions in the rest of this paper.

MEASUREMENTS NEAR TO THE TAMPED SHOT

The measurements near the tamped shot are easier to interpret since the signal is larger, but there are still some instrumental difficulties. The low frequency signal is from one-half to one-third that which would have been expected assuming that salt acts just like tuff. There is a second independent measurement based on the size of the cavity formed by the shot which agrees with the one-third figure. This smaller signal probably results from a smaller coupling in salt than in tuff for tamped shots. One would believe that a high explosive charge would give a greater signal than an equivalent yield nuclear shot. It, however, may be connected with some less significant effects such as the failure of the high explosive to give its full yield. The larger shots of phase II should throw some light on this subject.

CONCLUSIONS ON THE NEAR-IN MEASUREMENTS

I. The operation of the hole seems about as predicted.

II. The seismic coupling for a tamped shot in salt appears to be about one-half to one-third the coupling for a tamped shot in tuff. If this holds true for large nuclear explosions, it would be a significant fact.

III. Because of the weaker signal from the tamped shot in salt, the decoupling factor for low frequencies comparing a decoupled shot in salt to a tamped shot in salt appears to be in the neighborhood of 150.

IV. The prediction of a decoupling factor of 350 for a decoupled shot in salt to a tamped shot in tuff still appears to stand. Again, this is the important decoupling factor to know.

SURFACE MEASUREMENTS

The Cowboy experiments contain measurements by seismometers of the surface motions of the ground in the area around the explosion out to distances of a few kilometers. The signals which are recorded by the instruments contain mostly frequencies around 50 cycles per second. The earth absorbs frequencies much higher than this as the signal goes from the explosion to the receiver. The low frequencies are lost in the noise. The 50 cycles per second range is a particularly difficult one. There is no near-in data which is reliable in this range. It is also a range in which chemical explosives are expected to give larger signals than nuclear explosions for decoupled shots. This larger signal results from the large mass of high explosive hitting the wall. Further, the wavelengths at these frequencies are short enough that the various inhomogeneities in the area will affect in different ways the signals from the two shots (which are several hundred feet apart). Indeed seismometers only separated by a few hundred feet give quite different results. For these reasons the surface data cannot be used to give quantitative results at least at this stage of our analysis. (It is unfortunate that this range is so difficult in this experiment since the 50 cycles per second range is quite interesting. This is the range for 100-pound explosions that corresponds to the frequencies seen at large distances for large explosions.) The only firm conclusion that can be given at this time is that the surface data show a large decoupling factor but the numerical sizes of this factor have not been determined.

This report is only a preliminary one and it is expected that further work will yield more definite answers. Also, we expect the larger explosions of phase II to give more easily understood results.

GEN/DNT/TWG.2/9
December 18, 1959
English
Original: English/Russian

CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPON TESTS

Report of Tactical Working Group Two

Letter dated December 18, addressed to the Chairman of the Conference

December 18, 1959.

Dear Mr. Chairman: The first meeting of Technical Working Group Two was convened on November 25, 1959. Twenty-one official sessions of the group, all of them private, were held and work was completed on December 18, 1959.

The Conference members express their appreciation to the Secretary General of the United Nations and to his personal representative, Mr. T. G. Narayanan, for the facilities and secretariat services made available to them.

At the second meeting of Technical Working Group Two, November 26, 1959, the agenda of the Conference was adopted, as follows:

1. General statements.
2. Consideration, proceeding from the discussions and the conclusions of the Geneva Conference of Experts, of all data and studies relevant to the detection and identification of seismic events and of possible improvements of the techniques and instrumentation.
3. Consideration of the question of the use of the objective instrument readings in connection with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine a basis for initiating on-site inspections.
4. Preparation of a report containing conclusions on matters discussed above.

With reference to the approved agenda we note the following: On item No. 2 relating to new data and studies, there are areas of disagreement regarding the interpretation of the new data from the Hardtack experiments and regarding the question of decoupling. Agreement was reached on possible improvements of technique and instrumentation.

On agenda Item No. 3 there remain disagreements concerning the use of objective instrument readings in connection with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine a basis for initiating on-site inspections.

Conclusions on that portion of agenda Item No. 2 on which agreement was reached are attached to this letter as an annex. Separate reports and comments by separate delegations are also attached to this letter as annexes.

Respectfully,

U.S.S.R.: (Signed) E. K. Fedorov.
U.K.: (Signed) Sir William Penney.
U.S.A.: (Signed) J. B. Fisk.

Annex I—Conclusion of Technical Working Group Two Regarding Possible Improvements of Techniques and Instrumentation

A. The Technical Working Group has examined the following possible techniques and instrumentation which have the purpose of improving the detection and identification of seismic events:

1. *Arrays of seismographs.*—Arrays of 10 seismographs at each control post were considered and recommended by the Conference of Experts in 1958 in order to increase the signal-to-noise ratio. Experiments since then have confirmed the effectiveness of this method, and the possibility of further improvement by use of even larger arrays of seismographs.

2. *Use of long-period surface waves.*—Experiments since 1958 have shown that this method may assume an important role in the identification of earthquakes. The measurement of long-period waves would be facilitated by including, at all control posts, three-component long-period seismographs with response characteristics epitomized for the registration of such waves. These waves will have periods greater than about 10 seconds.

3. *Improvement of the operating characteristics of seismic equipment.*—It may be possible to develop seismographs with more nearly ideal response characteristics, both in the region of long and short periods, to improve the registration of seismic events in the presence of noise.

4. *Seismographs in deep holes.*—The method of using seismographs in holes sufficiently deep to avoid surface noise should be studied further and tested.

5. *Spectra of long-period surface waves.*—Preliminary studies of long-period surface waves show promise that, at a given station, earthquakes may be characterized by a lower dominant frequency than are explosions of the same magnitude in the same area.

6. *Other methods of potential interest.*—There is sufficient theoretical basis for the following methods to justify their further study, but experimental results, although encouraging, are insufficient at present for us to say exactly how they can be used. After some experience and additional research, some of the methods listed below might prove to be of considerable value to the control system.

(*a*) *Use of the ratio of the amplitudes of horizontal and vertical waves.*—The ratio of horizontal amplitude to vertical amplitude of short-period transverse body waves is related to detailed mechanisms at the source. This method could become important if the influence of departures from homogeneity, both at the source and along the propagation path, could be eliminated. There is good reason to expect that the ratio of the amplitudes of long-period waves (for example, Love waves and Rayleigh waves) will be less sensitive to these inhomogeneities and therefore more effective in revealing the characteristics of the source.

(*b*) *Analysis of long-period surface waves.*—Long-period surface waves were detected at large distances in the case of the underground nuclear explosions Blanca and Logan. In principle, information about the characteristics of the source, relevant to the problem of distinguishing earthquakes from explosions, can be obtained by analysis of these long-period surface waves. Partial success has been achieved in a few experimental investigations of earthquakes and explosions. Until the azimuthal variation of surface waves from different sources has been studied, the significance cannot be determined.

(*c*) *Comparison of the shapes of entire longitudinal wave trains received at different stations.*—This method offers hope of extending capability of direct assessment of first motion. Development of objective methods of comparison is needed in order to properly evaluate the possible importance of this method.

(*d*) *Use of modern data analysis techniques and electronic computing machines.*—The analysis of seismic waves can be improved by use of modern computing methods, both analog and digital, including digital recording and processing on calculating machines and certain advanced techniques of modern communication theory.

(*e*) *The use of reflected PP and other waves to provide data for the study of the nature of the source of the seismic event.*

B. The Technical Working Group recommends, on the basis of the consideration of the possible methods for improvement described in Section A above, that the recommendations of the Conference of Experts for instrumentation at control posts be modified as follows:

1. Installation at all control posts of three-component long-period seismometers of periods greater than 10 seconds with the response characteristics optimized for the registration of surface waves in the presence of noise.

2. Increase in the number of short-period vertical seismometers from 10 (as recommended by the Conference of Experts) to 100.

3. The choice of the optimum short-period seismometer as the component of the arrays recommended by the Conference of Experts for purposes of best registration of first motion in the presence of noise.

C. The Technical Working Group recognizes that the recommendations in Section B above represent improvements of instrumentation for the detection and identification of seismic events and that the possible methods considered in Section A may lead to improvements in the future.

D. The Technical Working Group recommends for consideration by the Conference the techniques and instrumentation described above, which have the purpose of improving the detection and identification of seismic events.

Annex II—Statement by the Soviet Experts

The Soviet experts examined, jointly with their United States and United Kingdom colleagues, all the material presented to the Technical Working Group. The Soviet experts considered that the Group's main task was, as indicated in its terms of reference, to develop proposals concerning the use of objective instrument readings as a basis for initiating on-site inspections. The Soviet experts believed that a necessary part of their work was to consider possible improvements of techniques and instrumentation. It was that purpose which the experts thought was to be served by the consideration and discussion of all data and studies relevant to the detection and identification of seismic events. In spite of the clearly defined terms of reference of the Technical Working **Group, the U.S. experts insisted that the Group should also consider the question** of estimating the effectiveness of the control system and should revise the estimates made in the report of the 1958 Geneva Conference of Experts. As grounds for revising those estimates, the U.S. experts cited the "new seismic data" they **had submitted. As the Conference knows, the Technical Working Group has adopted an agreed decision on the question of possible improvements of techniques and instrumentation, but failed to arrive at a common view on a combination of objective instrument readings which might serve as a basis for initiating**

on-site inspections, or on the importance of the "new seismic data" for estimating the effectiveness of the control system. The views of the Soviet experts on certain problems discussed by Technical Working Group Two are set forth in this statement.

The "new seismic data" were first published in the working paper of January 5, 1958 (document GEN/DNT/25). This document contains the following findings:

"Stations [used in the Hardtack tests] were all equipped with Benioff short-period vertical seismographs (described in the conclusions of the Geneva Conference of Experts)."

The data obtained in the course of investigation indicate that in the range of yields 0.1 to 23 kilotons the amplitude varies approximately as the first power of the yield of the explosion.

Earlier estimates of the seismic magnitude of the Rainier explosion were too high and new data are given on the magnitudes of the Rainier explosion and of explosions in the Hardtack series.

Previous estimates (at the 1958 Conference of Experts) of the number of earthquakes equivalent to explosions of a given yield require revision and should be approximately doubled.

The method of distinguishing earthquakes from explosions by the direction of first motion in the seismic signal is less effective than indicated in the Experts' report.

The annual number of unidentified continental earthquakes equivalent to 5 KT or larger will be greater than that estimated in the experts' report by a factor of 10 or more.

It must be noted that the document of January 5 provides virtually none of the original, factual data on which its findings are based. The few figures given were obtained by some kind of processing—generalization, analysis, selection—of the original data by the author. Fundamentally, the document is a statement, an expression of opinion by the author, and not a summary of original data on the basis of which any other investigators could draw their own conclusions. As is known, the publication of the January 5 document was the occasion for statements by many political figures of the United States of America and other Western countries—and particularly by the representatives of the United States and the United Kingdom at the Conference on the Discontinuance of Nuclear Weapon Tests—to the effect that the control system recommended by the Experts did not actually possess the capabilities laid down by the experts and should be reviewed. There has been a profusion of similar statements in the Western press. Their general theme has been the alleged ineffectiveness of the control system and the need to review the work done by the Experts in 1958.

In the Technical Working Group the U.S. delegation, in documents and statements, has repeated the findings contained in the working paper of January 5, 1959, with one small modification, viz, the number of earthquakes is raised by a factor, not of 2, but of 1.5.

Having examined the material presented, the Soviet experts have arrived at the inferences and conclusions set forth below.

INSTRUMENTATION USED IN HARDTACK TESTS

During the Hardtack II series of tests, not one of the stations situated in the United States used the array of seismic instruments recommended in the experts' report. Furthermore, not one of the seismographs used conformed in its parameters to the recommendations in the experts' report. This is easily established by comparing the parameters of the instruments used with the parameters recommended in the experts' reports. (See table, p. 76.)

Therefore, the assertion in the working paper of January 5, 1959, that all stations were equipped with Benioff short-period vertical seismographs, allegedly as described in the conclusions of the Geneva Conference of Experts, is a misrepresentation, as the U.S. delegation itself has virtually admitted.

It is obvious that these other and less efficient instruments could not produce the results that might have been expected if the arrays of seismic instruments recommended by the experts had been used.

The document of January 5 and the report to the Conference give the impression of a large-scale, well-conducted experiment. In actual fact, the situation was quite different. It was ascertained during the meetings of the group that of the dozens of seismic stations in the United States, only a small proportion possessed

instruments that satisfied, if not the recommendations of the Conference of Experts, even the elementary conditions demanded of any scientific instrument. For example, the Logan explosion was registered by 54 seismic stations in the United States. However, only about one-half of them (28) had properly calibrated instruments and, in the opinion of the U.S. seismologists themselves, seismograms from only 16 of these 28 stations could be used for analysis. It is indeed surprising that the instrumentation was not calibrated at least for the period during which these unique experiments were to be conducted.

MODIFICATIONS OF PRIMARY MAGNITUDES

Members of the U.S. delegation delivered to the group a large number of copies of seismograms. Quite obviously, in the short period of time available, the other delegations were unable to scrutinize and remeasure the amplitudes recorded, and they asked repeatedly for tables consolidating the source data. These were not supplied by the Americans till the 16th meeting.

The Soviet experts note that U.S. specialists have repeatedly modified the primary data submitted by them in support of various conclusions. For example, the magnitude of the Rainier explosion was first determined by U.S. specialists at the end of 1957 and (as reported in U.S. scientific publications in February 1958) was 4.6. At the 1958 Conference of Experts, U.S. specialists stated that the magnitude of 4.6 was incorrect and that the magnitude of the Rainier explosion was actually lower, i.e., 4.25. Finally, in the working paper of January 5, and in reports to the technical working group, U.S. specialists again reduced this magnitude to 4.07–4.06. The magnitude of the Tamalpais explosion has undergone similar perturbation. In one of the papers submitted by the U.S. experts at a meeting of the technical working group this magnitude is given as equal to 3.1, and in others to 2.6.

In support of its conclusions concerning the number of times the amplitude of first motion to smaller than the peak amplitude of a seismic signal, the U.S. delegation submitted its measurements of the amplitudes registered at certain stations. When the Soviet scientists stated that these measurements contradicted the above conclusion, some new values, never previously adduced in the U.S. documents, were presented at the 19th meeting. After close questioning, it was ascertained that the 0.1 ratio of first-motion amplitude to peak amplitude cited by the U.S. specialists had not been measured at all at stations in the 2,500- to 3,500-kilometer zone, although it is on the basis of this very figure that they arrive at some far-reaching conclusions.

Notwithstanding the fact that the initial processing, the preparation for analysis, and the analysis itself, of the seismic data had been done by persons who were U.S. nationals, the Soviet scientists tried to approach the material before them with the utmost confidence in its reliability, on the assumption that it was the result of careful scientific work. The above-mentioned manipulations of the primary data, the erroneous findings in the documents presented by the U.S. experts, and the fact that the instrumentation used for observing the tests was not in conformity with the 1958 Geneva recommendations, have compelled the Soviet scientists to adopt a more cautious attitude toward the quality and objectivity of the figures that were supplied by the Americans.

ESTIMATES OF SEISMIC MAGNITUDES OF EXPLOSIONS AND NUMBER OF EARTHQUAKES EQUIVALENT TO EXPLOSIONS OF GIVEN YIELD

On the basis of the data presented at the 16th meeting it was possible to establish that in the working paper of January 5, 1959, in computing the magnitudes of the explosions, the local magnitude M_L, obtained with the help of torsion seismometers in the first zone (the zone before the shadow zone), had been incorrectly identified with the unified magnitude m, obtained on the basis of observations in the third zone (the zone beyond the shadow zone). In computing the average magnitudes, magnitudes from different scales, M_L and m, were used without being reduced to uniform units. As a result of this, the average magnitudes supplied in the working paper were meaningless. Moreover, in the computation of the average magnitudes, the fact that magnitudes in the shadow zone are consistently undervalued was not taken into account. (This phenomenon in the shadow zone may be attributed to certain physicial causes.)

As a result of these and certain other erroneous operations, the working paper of January 5 produced greatly undervalued magnitudes for the Rainier, Logan, and Blanca explosions.

The Soviet experts did a more careful analysis of the seismic data on the Blanca and Logan explosions as well as for Rainier. This analysis was based on the well-known work on magnitudes of earthquakes and explosions done by Gutenberg and Richter between 1956 and 1958. As primary data the Soviet experts used all the magnitudes in the first (M_L) and third (m) zones (i.e., exclusive of the shadow zone) which had been presented by the U.S. delegation at the 16th meeting on December 14. All of the magnitudes were reduced to the unified scale of m.

As a result, the following magnitudes for the explosions were arrived at:

	Magnitude in working paper of Jan. 5, 1959	Corrected magnitude
Rainier (1.7 kilotons)	4.07±0.4	4.7 ±0.1
Logan (5 kilotons)	4.4 ± .4	4.95± .1
Blanca (19 kilotons)	4.8 ± .4	5.2 ± .1

Because of the incorrect magnitudes in the working paper of January 5 and in other documents of the U.S. experts, the formulation of the relationship between an explosion's magnitude and yield, given at meetings of the working group, was wrong. The more careful analysis of the Soviet experts based on more precise magnitude values results in a different relationship between the magnitude and yield of explosions in the 1.7- to 19-kiloton range.

On the basis of the more precise determination of this relationship and the Gutenberg statistics of 1956, it is possible to estimate the number of earthquakes per year throughout the world which are larger in magnitude than an explosion of given yield. These numbers are presented in the following table, together with the estimates of the 1958 Conference of Experts and the estimates given in the working paper of January 5.

Yield	Numbers of earthquakes equivalent to, or larger than explosions of given yield		
	Tentative estimates 1959	Geneva estimates 1958	Working paper of Jan. 5, 1959
(1)	(2)	(3)	(4)
1 kiloton	3,000	10,000	26,000
5 kilotons	1,500	3,800	5,800
10 kilotons	1,000	2,400	3,000
20 kilotons	800	1,500	1,600

Thus, on the basis of a more careful analysis of the new seismic data, the Soviet experts have come to the conclusion that the annual numbers of earthquakes throughout the world equivalent to explosions of a given yield are, if anything, smaller than the numbers estimated at Geneva in 1958 and not 1.5 or 2 times greater, as is asserted in the U.S. documents.

For computing the continental earthquakes only, the numbers in the above table must be reduced by a factor of 2.

SIGN OF FIRST MOTION

The assertion in paragraph 9e, of the working paper of January 5, 1959, that "the determination of the direction of first motion is much more difficult than anticipated," the assertion in paragraph 10a of the same document that "the method for distinguishing earthquakes from explosions by the direction of first motion is less effective than previously estimated," and similar statements by the U.S. delegation on this question in the present conference have not been sufficiently substantiated.

Although all the seismic stations of the special network were equipped only with single Benioff short-period seismographs (and not with arrays of 10 of the recommended seismographs) and, what is more, these instruments did not have the magnification of those recommended in the report of the Experts, they

nevertheless clearly recorded the first motion as a compression, in the case of the Logan (5 kilotons) and Blanca (19 kilotons) explosions up to epicentral distances of 700 kilometers.

If the instrumentation recommended in the Experts' report had been used in the Hardtack experiments, the first motion from the 5-kiloton explosion could have been clearly registered (with signal exceeding background by a sufficient margin) up to epicentral distances of 1,000 kilometers and, beyond the shadow zone, up to epicentral distances of 3,500 kilometers.

The report of the 1958 Conference of Experts contains the following statement concerning the possibility of distinguishing signals of explosions from those of earthquakes (equivalent to an explosion of 5-kiloton yield):

"5. The majority of earthquakes can be distinguished from explosions with a high degree of reliability if the direction of first motion of the longitudinal wave is clearly registered at five or more seismic stations on various bearings from the epicentre. Thus not less than 90 percent of all earthquakes taking place in continents can be identified. The remaining 10 percent or less of cases will require the analysis of additional seismograms where this is possible; and for this purpose use must also be made of the data of the existing network of seismic stations * * * (EXP/NUC/28, p. 11).

This conclusion is by no means refuted but rather confirmed by the measurements taken during the Hardtack experiments, if the quality and characteristics of the instruments used are duly and objectively taken into account.

The raising of the number of seismometers in arrays at each control post from 10 to 100 now being recommended will increase considerably (approximately threefold) the capacity of the control system to determine the direction of first motion in the first longitudinal wave, and this will also permit the identification of explosions of a lower yield.

SIGNIFICANCE OF THE NEW SEISMIC DATA

The Soviet experts believe that the seismic observations carried out in connexion with the Hardtack tests will certainly be of significance to the work of the control system. They will help the control system to improve its instrumentation and its measurement techniques, as well as in other ways; they will therefore have to be studied most carefully by the control organization staff concerned.

At the same time, and on the basis of careful analysis of all the material presented, the Soviet experts categorically repudiate the way in which primary data have been handled in the document of January 5 and by some members of the U.S. delegation in the Working Group. Having uncovered many errors as mentioned above, and even some misrepresentation, in U.S. statements and documents, the Soviet experts note that they all tend in a single direction—toward reducing the estimates of the control system's effectiveness. The Soviet experts therefore cannot regard these shortcomings as resulting from carelessness or coincidence, and have come to the conclusion that there has been tendentious use of one-sidedly developed material for the purpose of undermining confidence in the control system whose basic characteristics were determined by the 1958 Geneva Conference of Experts.

In this connexion, it must be noted that only the practical utilization of the control system will best permit evaluation of its effectiveness and introduction of improvements as appropriate. Many of the suggestions put before the Working Group by the U.S. scientists concerning possible improvements in the instrumentation and techniques of the control system can be regarded as correct in principle. Some of them could even now be recommended for practical application. Others may be applicable after a certain amount of experience has been gained in the operation of the control system. Here we would restate what the Experts said in 1958, namely that the use of techniques and instrumentation reflecting the latest scientific developments can always be proposed. Such innovations will improve the operation of the control system and increase its capabilities. It is clear from this that the next task is not to engage in endless discussion about the potentialities of the control system but to conclude an early agreement on the cessation of tests, establish a control system, and improve it on the basis of practical experience.

DISSIMILATION OF SEISMIC SIGNALS OF UNDERGROUND EXPLOSIONS

During discussion of the amplitude of the seismic signal produced by an underground nuclear explosion, the U.S. delegation introduced the idea that the

seismic signal could be disguised by carrying out the explosion in a sufficiently deep and large underground cavity. The U.S. experts' views on the possibility of considerably reducing the seismic signal under such conditions are, in the main, based on general considerations of theory. However, even from the theoretical point of view, the earth's crust is a very complex medium. Therefore, a combination of formal mathematical solutions for the problems involved in the dissimilation of underground explosions does not as yet offer a sound basis for any findings relating to the possible amplitude of the seismic signal generated by an explosion in a deep underground cavity or to the technical feasibility of carrying out vast underground construction operations at a depth of the order of 1 kilometer.

OBJECTIVE INSTRUMENT READINGS AS A BASIS FOR INITIATING ON-SITE INSPECTIONS

As the experts concluded in 1958, the control system will record a considerable number of seismic signals from natural earthquakes, some of which it will not be able to identify. Study of an unidentified event may give rise to suspicions and call for investigation at the site of the event. The Technical Working Group should elaborate criteria which would serve as a basis for initiating onsite inspection. As this statement has already indicated, the control system will record thousands of earthquakes equivalent to a yield of 1 to 2 kilotons and above. It is perfectly obvious that criteria must be chosen in such a way that, of this multitude of recorded seismic events, a relatively small number may be selected whose signals are consistent with those of nuclear explosions. Such events may be regarded as suspicious. It is therefore clear that, the more useful the criteria in permitting the selection of the most suspicious events, the more effective will be the work of the control system. If the control system regards all registered seismic events as equally suspicious, it will never be able to detect real violators.

The 1958 experts' report already contains clear and definite conclusions concerning the possibilities and methods of distinguishing the seismic signals of earthquakes from those of explosions. The agreed proposals of the Technical Working Group relating to improvements will unquestionably increase the control system's capability of correctly identifying earthquakes of less than 5 kiloton yield. In this connection, it would appear that the Technical Working Group was to concentrate its attention on perfecting the criteria previously laid down and on finding new criteria, thereby augmenting the effectiveness of the control system in carrying out its most important function, that of identifying suspicious events and detecting possible violators.

With that purpose in mind, the Soviet experts put forward specific principles for the selection of suspicious events on the basis of findings as to a combination of objective instrument readings.

The basic requirements for determining that seismic events are to be regarded as suspicious can be different in aseismic areas (which represent almost 80 percent of total continental area), where natural earthquakes are rare and control posts will be widely dispersed, and in seismic areas, where natural earthquakes are very frequent and the network of control posts will be considerably thicker. The Soviet proposals on criteria meet these requirements.

We consider that a seismic event localized within an aseismic continental area of 200 square kilometers (the experts' report establishes that, with well-defined coordinates, a seismic event can be localized within an area of 100 square kilometers) can be regarded as suspicious if there are no clear and definite indications that it has occurred at a depth of tens of kilometers or that it has been accompanied by the characteristic foreshocks and aftershocks of an earthquake.

Thus, the greater part of earthquakes occurring in aseismic areas would, in practice, be regarded as suspicious events.

This approach cannot be used with regard to seismic events occurring in seismic areas, where there are thousands of earthquakes per year. However, only some of them produce signals similar to those of nuclear explosions. The great density of the control-post network in seismic areas will make it possible to ascertain reliably differences in the nature of seismic signals. For this purpose, it will be necessary to utilize the distinguishing characteristics previously ascertained and some of those now proposed. Specifically, the Soviet representatives proposed the use of the sign of first motion of the seismic signal (the effectiveness of which was ascertained by the experts in 1958), as well as certain new indicators.

Such an approach will unquestionably insure the detection of truly suspicious events, on which the control system's future activity will be concentrated.

The approach of the U.S. delegation is completely different. On the basis of wrong conclusions already referred to and notwithstanding the facts, the U.S. experts suggest a system of criteria which virtually rejects the very idea of selecting suspicious events from the events recorded by the control system.

As a result of analysis, it became clear, for example, that if the U.S. criteria were used the determination of first motion could be considered basically reliable only in the case of explosions with yields of the order of hundreds or thousands of kilotons. Obviously, such a formulation of criteria is challenged even by the U.S. interpretation of the Hardtack measurements.

According to the U.S. scientists themselves, their criteria would leave under suspicion the overwhelming majority of the earthquakes registered by the control system.

The Soviet experts submit that here their U.S. colleagues are on the brink of absurdity.

In concluding this statement, the Soviet experts note that, in spite of the significant differences that still remain, the meetings of the Technical Working Group had a positive result, as is shown by the adoption of an agreed decision regarding improvements of techniques and instrumentation, which can increase the effectiveness of the control system.

TABLE

Comparison of parameters of control-post seismic apparatus as determined in report of 1958 Geneva Conference of Experts, with parameters of seismic apparatus used in Hardtack tests

Types of instruments and parameters required	Parameters of instruments used in Hardtack tests	Shortcomings in observations resulting from failure of parameters to meet requirements
1. Short-period vertical seismographs:		
(*a*) Approximately 10 should be dispersed over a distance of 1.5 to 3 kilometers.	Not 1 station was equipped with recommended number of seismographs, dispersed and connected as required for best signal to noise ratio and clear registration of 1st motion. Stations were equipped with only single vertical seismograph. Recommended method of arrays was used in only 2 cases, and then only 4 seismographs were installed.	Effective capacity to detect signal was considerably reduced; this led to wrong registration of 1st motion at some stations. In the 2 cases where arrays were used, even with only 4 seismographs a considerably greater ratio of signal amplitude to interference amplitude was obtained.
(*b*) Should have a maximum magnification of the order of 10^6 at a frequency of 1 c.p.s.	Seismographs had maximum magnification at a frequency of 3 c.p.s. Magnification of instruments was not raised to 10^6.	Maximum frequency response of seismograph did not fit maximum spectrum of signal when recorded in the 3d zone. Optimum recordings were therefore not obtained.
(*c*) Should have a receiving-band adequate to reproduce the characteristic form of the seismic signal.	Relevant receiving-band of Benioff seismograph was, from level of 0.7, approximately of order of 2, whereas a broader receiving band is needed for accurate reproduction of form of seismic impulse.	Since receiving band of instruments was optimal, seismic signals were recorded with strong distortions, so that there was considerable reduction of ratio of 1st motion amplitude to peak amplitude of seismic signal. Consequently, results in registration of 1st motion obtained during tests were less satisfactory than might have been expected.
2. Horizontal seismographs with the parameters indicated in point 1.	These seismographs were used at some stations.	
3. 1 3-component installation of long-period seismographs having a broad receiving band and a constant magnification of the order of 10^3 to 2×10^3 in the period range 1 to 10 seconds.	These seismographs were not used at a single station.	The vertical seismographs of this combination, if used as recommended, would have insured correct reproduction of form of seismic signal without reducing amplitude of 1st motion. Under favorable noise conditions, registration of sign of 1st motion on these seismographs should have yielded incomparably better results.
4. 1 3-component installation of seismographs with magnification of the order of 10^4 to 2×10^4 where T=2 to 2.5 seconds.	These seismographs were used in only 4 cases.	In combination with other instrumentation, these seismographs could have provided supplementary data for identification.

ANNEX III—REPORT BY THE UNITED KINGDOM DELEGATION FROM THE PROCEEDINGS OF TECHNICAL WORKING GROUP TWO

(1) The United Kingdom has no seismological data of its own on the underground explosion Rainier or on the underground explosions of the Hardtack series. The United Kingdom has not exploded a nuclear device underground.

(2) The United Kingdom delegation agrees with the delegations of the Union of Soviet Socialist Republics and the United States of America on the material given in the first annex. [Conclusion of Technical Working Group Two regarding possible improvements of techniques and instrumentation].

(3) With regard to Hardtack data on first motion, the opinion of the United Kingdom delegation is stated as follows.

The 1958 conference of experts concluded that the majority of earthquakes can be distinguished from explosions with a high degree of reliability if the direction of first motion of the longitudinal wave is clearly registered at five or more stations. They also stated that, using their recommended array of seismometers at each control post, the direction of first motion would be clearly observed from weapons greater than 5 kilotons over a range of distances from 2,000 to 3,500 kilometers from the weapon, even in periods of unfavorable noise conditions. This was the only instrumental criterion specifically mentioned by the experts and was considered by them to be sufficiently powerful to identify not less than 90 percent of all continental earthquakes.

The United Kingdom delegation believes that the Hardtack data have shown that the 1958 experts were too optimistic. For example, we have been impressed by the fact that only one station out of six in the third zone (2,500 to 3,500 kilometers) in the case of Blanca (19 kilotons) gave a clearly recognizable compressional first motion. Whilst it is true that these observations were made with single Benioff instruments, the use of arrays of 10 such instruments can be expected to have given one positively identified compressional first motion out of six stations from an explosion of 4-kiloton yield in the environment of the Blanca explosion. The use of 100 seismometers in each array, together with possible improvements in the individual instruments, would certainly increase the proportion of the stations in the third zone able to identify positively compressional first motion; but to what extent is not yet clear. Since the stations in the third zone are of prime importance for collecting first motion data, the United Kingdom delegation consider that even with arrays of 100 seismographs of improved design, it is unlikely that the stations in the third zone will have a high probability of being able to show a positively identified compressional first motion from an explosion of 5 kilotons in Rainier conditions.

The Hardtack data have been criticized on the grounds that the Benioff seismographs used in the experiment did not comply with the specification of the 1958 conference of experts. However, the United Kingdom delegation accepts the demonstration given by the U.S. delegation that the characteristics of the Benioff instrument are such that the fact that it did not comply with the specification would not make it inferior for the registration of the first motion in the presence of seismic noise.

The maximum amount of information must be extracted from the basic data of Rainier and Hardtack because these are the only underground nuclear explosions for which we have results.

(4) With regard to the number of earthquakes per year in the world equivalent to or greater than underground explosions of a given yield, the United Kingdom delegation have the following views.

The Geneva Conference of Experts in 1958 had available to them the data from only one underground nuclear explosion (Rainier). As a result of the Hardtack series, more data have become available. Using all of these data, we estimate that each year throughout the world there will probably be between 1,000 and 4,000 shallow earthquakes equivalent to 20 kilotons or more, between 2,500 and 10,000 equivalent to 5 kilotons or more, and between 7,500 and 30,000 equivalent to 1 kiloton or more. The spread in these figures reflects our uncertainties about the seismic signals generated by underground nuclear explosions, and also our ignorance of the worldwide distribution of small earthquakes.

We think that it is important to appreciate the large range of uncertainty in these numbers.

(5) With regard to the possibility of greatly reducing the seismic signals by exploding a nuclear weapon in a deep underground cavity, the views of the United Kingdom delegation are as follows.

Theory shows that the seismic signal generated by a nuclear explosion in an underground cavity can be much less than that from an equal explosion in Rainier conditions. The theory assumes that the cavity is sufficiently large and deep for the material around the cavity, right up to the boundary surface, to behave elastically. Calculations indicate, for example, that a cavity in salt, 1 kilometer below ground level, of volume 7×10^4 m^3 per kiloton, should suffice to meet the theoretical requirements.

The theory assumes a perfect isotropic medium, and makes certain assumptions about the stress distribution around the cavity before the explosion occurs. Careful work would have to be done before it could be stated that conditions likely to be met in practice do not require the theory to be modified. However, it may well be possible in practice, should the necessary scientific research and engineering effort be found, to give confidence that a decoupling factor of two orders of magnitude compared with Rainier conditions could be achieved.

Experiments have been made to compare the seismic signals at about 1 mile from high explosive charges, exploded in an underground cavity, with those given by smaller charges placed a few feet from the cavity in the surrounding limestone. Approximately 10 times as much explosive was required in the cavity as in the surrounding limestone to give equal seismic signals. A decoupling factor of about one order of magnitude in limestone has thus been demonstrated for one cavity which was smaller and less deep than those demanded by the above-mentioned theory.

Cavities are known to exist in salt formations which would satisfy the volume and depth requirements for a few kilotons. The cavities are not spherical shape and the stress distributions around the cavities are not known.

(6) The Soviet delegation suggested criteria which would separate off a relatively small class of events which they consider eligible for inspection. In our opinion, explosions of several tens of kilotons would be correctly classified by these means; but small explosions would, with high probability, be misidentified as earthquakes.

The U.S. delegation suggested criteria which will identify most large earthquakes, but leave the majority of seismic events unidentified and hence eligible for inspection.

We accept the value of the American and Soviet criteria on depth of focus and epicentral location in deep oceans. We think that the criteria on foreshocks and aftershocks are useful ones and we also approve the proposed criterion about eligibility for inspection based on the location of the epicentre in an aseismic area.

With regard to the location of an epicentre, we consider that when reliable travel time curves have been established, it should be possible to locate an event to within an area of 200 square kilometers, except near a coastline. National seismic stations can give valuable help in determining travel time curves.

In addition to the main criteria based on first motion, the Americans and the Soviet delegations have suggested a number of phenomena and methods capable of giving auxiliary information. These may be called aids to criteria. We believe that these aids can appreciably increase the value of the choice of an inspection over that of random choice. We are hopeful that some may be capable of being codified into valuable criteria at some time in the future, when the necessary research has been done. However, we cannot at present see how any of these aids can be codified, and we do not believe that at present any of them has the element of reliability that a criterion must have.

Thus the difficulty in which we find ourselves is that we cannot at present propose criteria which will identify a large proportion of earthquakes as such. A large number remains unidentified and must therefore be considered as eligible for inspection. Aids are not yet in a codified form or sufficiently reliable to be used as criteria but they do have real value as statistical weighting factors.

In our opinion, the best that can be done in the present state of knowledge is to define criteria which will identify a modest proportion of earthquakes. The remaining events must be regarded as eligible for inspection, even though the number of events will, for a few years, be far too large for all to be inspected. However, world research on seismology, including work done by the control organ itself, could in a few years materially increase the proportion of earthquakes which can be identified.

It is for the political conference to consider the procedure by which the aids to criteria might be introduced as weighting factors and the procedure by which the events to be inspected are chosen.

(Signed) W. G. PENNEY,
Head of United Kingdom Delegation to Technical Working Group Two, Geneva.

DECEMBER 18, 1959.

ANNEX IV.—REPORT OF THE DELEGATION OF THE UNITED STATES CONCERNING TECHNICAL WORKING GROUP TWO OF THE CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPONS TESTS

A. The Conference on the Discontinuance of Nuclear Weapons Tests on November 24, 1959, agreed upon terms of reference for Technical Working Group Two as follows:

"The Technical Working Group of experts shall consider the question of the use of objective instrument readings in connection with the selection of an event which cannot be identified by the international control organ and which could be suspected of being a nuclear explosion, in order to determine a basis for initiating onsite inspections. As part of their work, the experts, proceeding from the discussions and the conclusions of the Geneva Conference of Experts, shall consider all data and studies relevant to the detection and identification of seismic events and shall consider possible improvements of the techniques and instrumentation."

B. The Delegation of the United States to Technical Working Group Two concludes on the basis of its consideration, proceeding from the discussions and the conclusions of the Geneva Conference of Experts, of all data and studies relevant to the detection and identification of seismic events as follows:

1. On the basis of the Hardtack data, the amplitude of first motion of P-waves is smaller than was concluded on the basis of data available to the Conference of Experts. Because of the small amplitude of first motion and the presence of noise, the first motion from an explosion will sometimes appear to be a rarefaction at distances up to 1,100 kilometers and also beyond 2,500 kilometers. The vertical component seismograms obtained during the Hardtack operation showed numerous apparent first motions corresponding to rarefactions. The direction of the first motion is also unreliable in the "shadow zone" which was shown by Hardtack data to extend from about 1,100 to 2,500 kilometers, instead of 1,000 to 2,000 kilometers as was concluded at the Conference of Experts. As a result of these new data concerning first motion, we conclude that the direction of first motion which the Conference of Experts considered to be the only specific criterion for identifying earthquakes is useful only at substantially higher equivalent yields than was concluded by the Conference of Experts.

2. The amplitudes of seismic signals produced in the Hardtack Series were smaller than would have been estimated on the basis of the data available to the Conference of Experts. We estimate that each year there will be throughout the world be about 2,000 shallow earthquakes equivalent to 20 Kt or more, about 5,000 equivalent to 5 KT or more, and about 15,000 equivalent to 1 KT or more. However, our knowledge of the frequency of occurrence is uncertain by at least a factor of two either way. It is expected that variations of location, medium, and depth of the explosion will also produce considerable variation of the amplitude of the seismic signal from a closely confined explosion of a given yield. Our estimated number differs from the number which could have been estimated on the basis of the data available to the Conference of Experts by less than the uncertainties mentioned above.

3. Theory now indicates that the amplitude of the seismic signal produced by an underground explosion at large distance should vary with the first power of the yield up to at least 20 KT. For sufficiently greater yields theory shows that the signal should vary as the two-third power of the yield. The best fit with a single power law to the Rainier and Hardtack data, including explosions in the 100-ton range, is provided by an exponent of 0.9 ± 0.1. Consideration of Rainier, Logan, and Blanca (1.7 to 19 kilotons) alone would indicate an 0.7 ± 0.1 power law.

4. The Hardtack tests show that strong horizontally polarized transverse waves (SH) and long-period Love waves are excited by underground explosions. Therefore, such waves cannot be considered characteristic only of earthquakes.

5. Rigorous theoretical calculations combined with measurements on the Rainier explosion show that the seismic signal produced by an explosion in a sufficiently large underground cavity in salt or hard rock will be reduced by a factor of 300 or more relative to the seismic signal from an explosion of the same yield under Rainier conditions. Calculations indicate that a cavity at a depth of about one kilometer with a volume of 7×10^4 cubic meters per kiloton should suffice.

Cavities are known to exist in salt formations which would satisfy the volume and depth requirement for explosions of several kilotons. Engineering studies indicate that it is feasible to construct cavities which would satisfy the volume requirement for explosions at least as large as 70 kilotons. The total construction time for a cavity of this size in a salt dome is estimated to be from 2 to 4 years.

C. The Delegation of the United States concurs with the conclusions and recommendations of Technical Working Group Two regarding possible improvements of techniques and instrumentation which have the purpose of improving the detection and identification of seismic events. These agreed conclusions and recommendations are set forth in the following paragraphs C 1 through C 4:

1. The Technical Working Group has examined the following possible techniques and instrumentation which have the purpose of improving the detection and identification of seismic events:

a. Arrays of seismographs.—Arrays of 10 seismographs at each control post were considered and recommended by the Conference of Experts in 1958 in order to increase the signal-to-noise ratio. Experiments since then have confirmed the effectiveness of this method, and the possibility of further improvement by use of even larger arrays of seismographs.

b. Use of long-period surface waves.—Experiments since 1958 have shown that this method may assume an important role in the identification of earthquakes. The measurement of long-period waves would be facilitated by including, at all control posts, three-component long-period seismographs with response characteristics optimized for the registration of such waves. These waves will have periods greater than about 10 seconds.

c. Improvement of the operating characteristics of seismic equipment.—It may be possible to develop seismographs with more nearly ideal response characteristics, both in the region of long and short periods, to improve the registration of seismic events in the presence of noise.

d. Seismographs in deep holes.—The method of using seismographs in holes sufficiently deep to avoid surface noise should be studied further and tested.

e. Spectra of long-period surface waves.—Preliminary studies of long-period surface waves show promise that, at a given station, earthquakes may be characterized by a lower dominant frequency then are explosions of the same magnitude in the same area.

f. Other methods of potential interest.—There is sufficient theoretical basis for the following methods to justify their further study, but experimental results, although encouraging, are insufficient at present for us to say exactly how they can be used. After some experience and additional research, some of the methods listed below might prove to be of considerable value to the control system.

(1) *Use of the ratio of the amplitudes of horizontal and vertical waves.*—The ratio of horizontal amplitude to vertical amplitude of short-period transverse body waves is related to detailed mechanisms at the source. This method could become important if the influence of departures from homogeneity, both at the source and along the propagation path, could be eliminated. There is good reason to expect that the ratio of the amplitudes of long-period waves (for example, Love waves and Rayleigh waves) will be less sensitive to those inhomogeneities, and therefore more effective in revealing the characteristics of the source.

(2) *Analysis of long-period surface waves.*—Long-period surface waves were detected at large distances in the case of the underground nuclear explosions Blanca and Logan. In principle, information about the characteristics of the source, relevant to the problem of distinguishing earthquakes from explosions, can be obtained by analysis of these long-period surface waves. Partial success has been achieved in a few experimental investigations of earthquakes and explosions. Until the azimuthal variation of surface waves from different sources has been studied, the significance cannot be determined.

(3) *Comparison of the shapes of entire longitudinal waves trains received at different stations.*—This method offers hope of extending capability of direct

assessment of first motion. Development of objective methods of comparison is needed in order to properly evaluate the possible importance of this method.

(4) *Use of modern data analysis techniques and electronic computing machines.*—The analysis of seismic waves can be improved by use of modern computing methods, both analogue and digital, including digital recording and processing on calculating machines and certain advanced techniques of modern communication theory.

(5) *The use of reflected PP and other waves to provide data for the study of the nature of the source of the seismic event.*

2. The Technical Working Group recommends, on the basis of the consideration of the possible methods for improvement described in paragraph C1 above, that the recommendations of the Conference of Experts for instrumentation at control posts be modified as follows:

a. Installation at all control posts of three-component long-period seismometers of periods greater than 10 seconds with the response characteristics optimized for the registration of surface waves in the presence of noise.

b. Increase in the number of short-period vertical seismometers from 10 (as recommended by the Conference of Experts) to 100.

c. Choice of the optimum short-period seismometer as the component of the arrays recommended by the Conference of Experts for purposes of best registration of first motion in the presence of noise.

3. The Technical Working Group recognizes that the recommendations in paragraph C2 above represent improvements of instrumentation for the detection and identification of seismic events and that the possible methods considered in paragraph C1 may lead to improvements in the future.

4. The Technical Working Group recommends for consideration by the Conference the techniques and instrumentation described above, which have the purpose of improving the detection and identification of seismic events.

D. The Delegation of the United States to Working Group Two concludes that any seismic event, located as described in paragraph D1 below, could be suspected of being an underground nuclear explosion and therefore should be eligible for inspection unless it meets one or more of the specific criteria enumerated in paragraph D2 below which identify the event as an earthquake. A seismic event occurring in an aseismic area is to be considered eligible for inspection even if it meets the criteria of paragraph D2(*d*) below. For this purpose an aseismic area is to be considered as any area at a distance of more than 100 kilometers from the epicenter of a located earthquake of magnitude 4.4 or greater.

1. *Location of seismic events.*—A seismic event shall be considered to be located when seismic signals, whose frequencies, amplitudes, durations, and velocities are consistent with those of waves from earthquakes or explosions, are recorded at a sufficient number of control posts to establish the approximate time and position of the event. This requires at least four clearly measurable arrival times of identifiable phases which are mutually consistent to within ±3 seconds. These four consistent arrival times must include P–wave arrival times at three different control posts.

If the event is eligible for inspection, the area eligible for inspection consists of that 500 square kilometers which has the highest likelihood of containing the epicenter, according to a previously established data reduction procedure. This procedure shall take into account the frequent occurrence of relatively aberrant observations and the relative accuracies of different observations. When adequately precise regional travel time curves are developed and used, and consistent arrival times are available from control posts surrounding the epicenter, that is, from control posts at least one of which lies in every possible 90° sector around the epicenter, the area eligible for inspection will be the 200 square kilometers which has the highest likelihood of containing the epicenter.

2. *Identification of earthquakes.*—A located seismic event shall be ineligible for inspection if, and only if, it fulfills one or more of the following criteria:

a. Its depth of focus is established as below 60 kilometers;

b. Its epicentral location is established to be in the deep open ocean and the event is unaccompanied by a hydroacoustic signal consistent with the seismic epicenter and origin time;

c. It is established within 48 hours to be a foreshock by the occurrence of a larger event of at least magnitude 6 whose epicenter coincides with that of the given event within the accuracy of the determination of the two epicenters. The eligibility of the second event for inspection must be determined separately;

d. The directions of clearly recorded first motions define a pattern which strongly indicates a faulting source. First motions recorded at distances between 1,100 kilometers and 2,500 kilometers will not be used. First motions beyond 3,500 kilometers will not be used for events of magnitude smaller than 5.5. The apparent direction of first motion must also meet both of the following minimum conditions to be considered to be clearly recorded:

(1) The amplitude of the half-cycle of apparent first motion is at least two times as large as any half-cycle of apparent noise in the preceding few minutes, and

(2) The largest of the amplitudes of the half-cycle of apparent first motion and the two immediately following half-cycles—

(*a*) at epicentral distances less than 700 kilometers is 20 times larger than any half-cycle of noise in the preceding few minutes;

(*b*) at epicentral distances more than 700 kilometers is 40 times larger than any half-cycle of noise in the preceding few minutes.

A pattern of clearly recorded first motions strongly indicates a faulting source if the observed motions, extended backward to a small sphere about the focus, can be separated into alternate quadrants by two orthogonal great circles drawn on the small sphere, with the requirement that two opposite quadrants combined (*i*) contain at least four clearly recorded rarefactive first motions and (*ii*) contain not more than 15 percent compressions among the clearly recorded first motions.

First motion criteria cannot properly be defined without specifying the seismograph used. In this connection the description of the short-period vertical seismographs which make up the arrays of 10 described by the Conference of Experts has to be expanded as follows: The instruments will be optimized to detect first motion in the field of noise known to exist in the earth and the receiving band will be selected to balance the need to reproduce the characteristic form of the seismic signal against the need to reject the noise, and will be such as will permit operation of single seismographs at quiet stations with a magnification greater than 10^6 at the frequency of peak response. These conditions are presently judged to place the frequency of peak response somewhere between 1 and 5 cycles per second.

e. It is established to be an aftershock of a seismic event of at least magnitude 6 which has been clearly identified as an earthquake by the criteria in paragraphs D2 *a*, *b*, or *d*. For this purpose an aftershock is defined as one of a sequence of earthquakes which occurs less than 1 week after the main shock and which has an epicenter within 10 kilometers of the epicenter of the main shock.

3. The basic data for all criteria will be obtained from the control posts. Supplementary data not involved in determining the eligibility of a particular event for inspection may be derived from national stations; for example, readings from national stations might be used to establish the regional travel time curves as described in paragraph D1.

E. The criteria described above in section D, which make certain detected and located seismic events ineligible for inspection, leave a large number of seismic events eligible for inspection. Selection of certain eligible events for inspection may be made more effective by the use of auxiliary seismological information, so long as this information is used only as an aid to selection, and is never allowed, by its own weight, to exclude the possibility that a particular event will be inspected. At the present state of seismological knowledge, the effectiveness of selection would be very greatly reduced if any rigid or formal procedure of using such auxiliary information were adopted. Such auxiliary information may include the following seismic phenomena:

1. Even if an event remains eligible for inspection in accordance with the criterion of paragraph D2d above, first motion phenomena may provide auxiliary information. Specifically, both (*a*) patterns of first motion consistent with a faulting source, and (*b*) rarefactional first motions at stations where the signal-to-noise ratio is high, are suggestive of natural earthquakes.

2. Surface wave phenomena can provide auxiliary information, particularly in view of their large amplitudes in comparison with first movements of the longitudinal wave. Specifically, the following are suggestive of natural earthquakes:

(*a*) inferred near-source behavior of Rayleigh waves consistent with a faulting source;

(*b*) high ratios of Love wave amplitude to Rayleigh wave amplitude at a number of stations at widely different azimuths from the source;

(*c*) predominant frequencies of Love waves equal to or less than the value typical of earthquakes of the same magnitude recorded over a similar path;

(*d*) predominant frequencies of Rayleigh waves equal to or less than the value typical of earthquakes of the same magnitude recorded over a similar path;

(*e*) unusually large amplitude of long-period surface waves in comparison to that expected of an explosion of the same magnitude, and

(*f*) strong dependence of long-period Rayleigh wave amplitudes on azimuth of observation.

3. The following also provide auxiliary information suggestive of a natural earthquake.

(*a*) a sequence of many aftershocks;

(*b*) large ratios of SH to SV amplitudes at many and widely different azimuths from the source;

(*c*) a seismogram duplicating in detail a seismogram from a known earthquake with coincident epicenter; and

(*d*) seismic waves, particularly surface-reflected body phases, indicative of a focal depth of tens of kilometers.

4. For the Nevada underground explosions and many earthquakes, at distances beyond 2,500 kilometers, the ratio of second-to-first motion appears to be several times larger, on the average, than the ratio of third-to-second motion. This fact may be useful in evaluating the direction of first motion, since on a given seismogram the earliest motion of a size comparable with that immediately following would then be likely to be the actual second motion. The variations in the ratios of second-to-first and third-to-second motion are large, and explosions in other situations may produce seismic waves of quite different amplitude ratios, so that the power of this method cannot be evaluated at this time.

It is not possible at the present time to devise a formal procedure for the use of this auxiliary information, nor is it possible to use it as a means to eliminate the eligibility of any particular event for inspecton. However, some of the seismic characteristics listed above may, in the future, develop into criteria to establish an event as natural in origin. The formulation and establishment of future criteria using such characteristics can only be carried out on the basis of continuing research. This research is likely to involve observations at many locations of earthquakes and, in some cases, of large explosions, the development of instruments, and the use of modern data-processing techniques.

ANNEX IV—APPENDIX

COMMENTS BY THE DELEGATION OF THE UNITED STATES REGARDING THE "REPORT OF THE DELEGATION OF THE UNITED STATES CONCERNING TECHNICAL WORKING GROUP TWO OF THE CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPONS TESTS"

In accordance with the Terms of Reference, the U.S. Delegation bases its report on all scientifically valid conclusions concerning the detection and identification of nuclear events based on new studies and data, whether such conclusions would lead to improvements of the system or would lead to an assessment which would make the system appear less effective. It is the view of the U.S. Delegation that mentioning only the potential improvements in its final conclusions on this subject would mislead the Conference on the Discontinuance of Nuclear Weapons Tests concerning the present technical status of the possible control system.

One of the important conclusions in Section B of the report deals with the so-called first motion problem; that is, the direction in which a seismic needle would swing as a first response to a seismic disturbance. This direction was considered by the Conference of Experts to be the primary tool for discriminating between earthquakes and explosions. The conclusion drawn by the U.S. Delegation based on new data is that this method of discrimination is much less effective than had been thought.

A further important conclusion in this section concerns the possibility of concealment of underground nuclear explosions by detonating such an explosion in a very large underground cavity in salt or hard rock. It was shown theoretically that the seismic signal of a given explosion under these conditions

could be reduced 300-fold or more as compared to the signals produced in the Nevada tests. Consequently explosions could be made to look smaller by this factor and thus be much harder to detect and locate.

An item in section B deals with the estimate of the number of earthquakes which would be expected to be detected and located by the control system. The conclusion is that the estimates of such a number are very uncertain but that about 15,000 earthquakes per year would be located by this system over the whole world, corresponding to earth movements produced by nuclear explosions of more than 1 kiloton. For larger explosions such as 20 kilotons, the number of equivalent earthquakes is about 2,000 worldwide.

All delegations concurred in section C, which contains a list of new seismological techniques which could in the future be used to improve the control system. Our knowledge of these techniques ranges from very detailed to purely speculative, but it is clear that further research work will demonstrate the degree of utility which these techniques and improvements of instrumentations may have on the control system. As a result of these studies, the delegations were able to agree on specific recommendations for instrumental improvements of the control system as listed in section C.

Section D describes "criteria based on objective instrument readings" which could be used by the control organization in determining the eligibility of detected and located seismic events for inspection. Agreement was not reached on this section. It is the view of the U.S. delegation that such criteria must be formulated so that a large number of explosions would not be classified as natural earthquakes and that the criteria must be based on well-established technical information. Unfortunately, the resulting criteria classify only a small fraction of the seismic events as natural earthquakes leaving a large number eligible for inspection. It was the view of the U.S.S.R. delegation that criteria must be specified by this working group which would remove a large fraction of the seismic events from eligibility for inspection by identifying them as natural earthquakes. However, it is the view of the U.S. delegation that this is impossible within present technical knowledge. In fact the criteria proposed by the U.S.S.R. delegation would have classified such events as the recent U.S. underground nuclear test explosions which range up to 19 kilotons in yield as natural earthquakes and thus would have made them ineligible for inspection. It is the view of the U.S. delegation that as scientific knowledge progresses, more useful criteria can be formulated in the future.

The U.S. delegation recognizes that there is a great deal of additional seismic information available, as listed in section E, which is not sufficiently complete to be formulated into specific criteria. The U.S. delegation feels that such auxiliary information should be very useful if evaluated in a competent technical manner in connection with a particular seismic event.

In the view of the U.S. delegation, the problem of the formulation of criteria is a strictly technical problem. If technical knowledge permits one to identify a large fraction of seismic events as earthquakes then it is clearly a great advantage to the control system. If technical knowledge does not permit this, then seismic events must remain eligible for inspection. Determination of the means of selecting events to be inspected must be left for further consideration by the Conference.

CONFERENCE ON THE DISCONTINUANCE OF NUCLEAR WEAPONS TESTS

List of Preamble, Treaty Articles, and Annexes Agreed To by the Three Nuclear Powers as of January 20, 1960

Preamble (adopted, April 17, 1959).

Article 1—Commitment to prohibit tests and not to give assistance to fourth countries (adopted, December 6, 1958).

Article 2—Twin obligation to establish and abide by control system (adopted, December 8, 1958).

Article 3—Structure of control organization (adopted, December 12, 1958).

Article 4—Composition of control commission (adopted, December 17, 1958).

Article—Installation of system in parties' territories (adopted, May 8, 1959).

Article—Undertakings concerning cooperation with the system (adopted, April 27, 1959).

Article—The Conference (adopted, May 8, 1959).

Article—Privileges and immunities (adopted, May 6, 1959).
Article—Relationships with other international organizations (adopted, April 16, 1959).
Article—Amendments (adopted, April 14, 1959).
Article—Periodic review of the system (adopted, March 19, 1959).
Article—Duration (adopted, March 19, 1959).
Article—Parties to the treaty (adopted, April 24, 1959).
Article—Signature, ratification, acceptance and entry into force (adopted, May 4, 1959).
Article—Registration (adopted, March 19, 1959).
Article—Annexes to be integral part of treaty (adopted, April 21, 1959).
Article—Authentic texts (adopted, April 29, 1959).
Annex III—The preparatory commission (adopted, November 30, 1959).

LIST OF DRAFT ARTICLES SUBMITTED BUT NOT YET AGREED TO AS OF JANUARY 20, 1960

(Listed in order of introduction)

1. Procedures of Nuclear Weapons Test Control Commission (U.K., December 8, 1958).
2. Functions of the Commission (United States, January 1959).
3. Administrator and international staff (United States, February 25, 1959).
4. Finance (United States, April 27, 1959).
5. Detonations for peaceful purposes (United States, January 30, 1959; U.S.S.R., February 23, 1959).
6. Annex I. Detection and identification system (United States, December 16, 1958).
7. Annex II on privileges and immunities (United States, December 7, 1959).

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